AMERICAN POLITICAL SCIENCE REVIEW

MARCH 1986
VOLUME 80 NUMBER 1

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The *American Political Science Review* appears in March, June, September, and December of each year. It is published by the American Political Science Association, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036, and sent to all members. Dues: Regular Members with income: under $20,000, $40; $20,000-$29,999, $50; $30,000-$39,999, $65; $40,000 and over, $75; Student Members (limited to 5 years), $15; Retired Members, $20; Life Members, $1,000; Institutional Members, $75. Dues allocated for a subscription to the *APSR*: $10. Changes of address sent to the Membership Secretary of the APSA. Postmaster: Please send notification regarding undelivered journals to address above. Second-class postage paid at Washington, D.C. and at additional mailing offices. Copyright © 1986 by The American Political Science Association (ISSN-0003-0554).

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POLITICAL
SCIENCE
REVIEW

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EFFECTIVE JUNE 1, 1986

All new manuscripts should be sent to:

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American Political Science Review
Department of Political Science
Ohio State University
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QUARTERLY JOURNAL OF THE
AMERICAN POLITICAL SCIENCE ASSOCIATION

MARCH 1986
VOLUME 80 NUMBER 1

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ARTICLES

OBSERVATION, CONTEXT, AND SEQUENCE IN THE STUDY OF POLITICS

RICHARD F. FENNO, JR.
University of Rochester

Observation is at the heart of political analysis. Observing the behavior of U.S. senators involves watching them in two contexts—at home and in the capital city. It also entails sensitivity to the sequences of events or contexts which impinge upon senatorial behavior. Contexts and sequences of legislative life have not been observed in the rich detail they deserve, because not enough political scientists are presently engaged in observation.

All students of politics are, perforce, students of politicians. Whether we place them near the center or at the periphery of our work, we cannot avoid thinking about those people who, in any society, pursue public careers, make public decisions, and enmesh themselves in public values. There are, of course, many ways to study politicians. I wish to talk about one of them: observation. By observation, I mean following politicians around and talking with them as they go about their work. Because I shall draw upon my experience in observing some United States senators, I shall speak most directly to people with an interest in American legislative politics. But I should like, in the course of my remarks, to engage all those who have an interest in what politicians do and why. Hence the pretentiousness of my title. The main question I wish to pose is, What, if any, value can the close personal observation of politicians bring to our studies of politics? A secondary question is, Should we, as a discipline, encourage more of it?

The observation of which I shall speak is, for lack of a better term, interactive observation. It is not like looking through a one-way glass at someone on the other side. You watch, you accompany, and you talk with the people you are studying. Much of what you see, therefore, is dictated by what they do and say. If something is important to them, it becomes important to you. Their view of the world is as important as your view of that world. You impose some research questions on them; they impose some research questions on you. That interaction has its costs—most notably in a considerable loss of control over the research process. It also has benefits. It brings you especially close to your data. You watch it being generated and you collect it at the source. It is not received data. Furthermore, these data—the perceptions, the interpretations, and the behavior of working politicians—are data that bring you close to serious political activity. This immediate proximity to data about serious political activity produces sensitivities and perspectives which, I shall argue, do give observation some "value added"—almost certainly for students of American legislative politics, and quite possibly for all political scientists who think about politicians.

We are not talking here about a theory of politics. We are talking about a mode of research. But it is a mode of research which can—potentially at least—inform, enrich, and guide theories of politics. Its potential may be greater for some bodies of theory than for others. The very activity of observing politicians carries with it a bias in favor of individual-level theorizing. It is individuals you are watching and it is generalizations about individuals that become the building blocks of your analysis. My own view begins with the idea that politicians are both goal-seeking and situation-interpreting individuals. It proceeds to the idea that politicians act on the basis of what they want to accomplish in their world, and on the basis of how they interpret what they see in that world. It moves from there to the idea that we can gain valuable knowledge of their actions, perceptions, and interpretations by trying to see their world as they see it. And hence to observation. I shall make a minimum claim for it—that it brings value added to individual-level analysis. But I hope that others might explore its further reaches for possible contributions to more macro-level theorizing.

There are, I shall argue, two large lessons to be drawn from observation. Each flows directly from a basic condition of observation as a research mode. First, in order to observe politicians, you must leave the place where you live and work, where routines and people are familiar, and go to some other place where you intrude—more or less—upon the lives, the work, and the routines of less familiar people. In order to observe politicians, you must operate in an unfamiliar context. And that basic condition drives you, inevitably, to a sharpened appreciation of *context* as a variable in your analysis.

The relevance of context becomes increasingly evident as you move from the observation of one politician to the observation of another. You are driven to ask, in each new encounter, Who is this person I'm with? What is he or she trying to accomplish? What is the situation in which he finds himself? How do he and his situation differ from that of other people I have observed? The object of your attention is "this person." But unless you are engaged in the study of individual psyches—as most of us are not—the unit of analysis is always "this person in this situation." You face an individual who is pursuing certain goals, holding certain personal attitudes and values, carrying a residue of personal experience. But you also face an individual who is perceiving, interpreting, and acting in a complex set of circumstances. And you cannot know what you want to know about that individual until you have knowledge of these "circumstances," or this "situation," or, this "context." By knowledge, I mean to include what you learn by looking at the context yourself and what you learn by seeing the context through the eyes of the individual politician. Observation involves interviewing; but it involves much more than interviewing.

There are two master contexts in which all legislative politicians work—home and the capital city. They can be observed in both places. I began by observing some United States senators at home. That experience conveys, overwhelmingly, the importance of context.

During my first two days with a senator from a large state, he held consecutive meetings with a conference of black ministers, a conservative citizens' group, representatives of two dozen national PACs, officials of the statewide Catholic Conference, a group of Jewish community leaders, the political operatives of the State Education Association, and officers of the Teamsters Union. Three weeks later, I spent my first full day with a small-state senator, driving across vast farmlands for a series of leisurely visits with groups of citizens in three small-town cafes. I saw the meetings in the large state as impassioned importunings, with various groups interviewing a prospective advocate for their cause. I saw the meetings in the small state as easygoing exchanges, with each group reassuring itself that the visitor was "one of us."

The contextual contrast I saw fit with each senator's own perceptions and interpretations. The large-state senator sees himself working in a contentious context, characterized by a great diversity of organized and insistent groups. He calls his state "a microcosm of the United States." Cultivating it, he says, presents "the same problem that a lawyer with a general practice has, of handling many clients without a specialty. It's the problem of dealing with hundreds, even thousands of interests." The small-state senator sees himself as working in a context dominated by rural-agricultural interests. He cannot escape "a specialty." "In [my state]," he says, "we either farm or we farm the farmers." And he sees no microcosm. He speaks, instead, about the uniqueness of his state, historically, culturally, and environmentally. As for cultivating it, he says, "When they accept you as family, it's easy." To observe senators at home is to become acutely aware that the observation of politicians does indeed, involve "this person in this context." The constituency context, is of course, only an example.

The second large lesson of observation also derives from a basic condition of the research mode. Because you are an outsider in every encounter, and because of the accompanying problems of access, observation is always episodic, never continuous. Your research is, by nature, drop-in-drop-out-drop-in again research. Your observations get made at irregular intervals and at numerous points in time. You are driven by that condition to an extra appreciation of time. You see not only "this person in this context," but "this person at this time." Attention to time leads, in turn, to an emphasis on the changes that take place over time. Contexts change. And observations made "at this time" will likely differ from those made "last time" or "next time." Finally, any set of serial observations produces a special sensitivity to the sequential aspects of change. Your observation "this time" is embedded in a sequence of "other times." Much of what is important about time and change can be captured in the study of sequences. Some observed sequences exhibit regularities; they seem to have a causal logic to them. They are the most easily and advantageously studied. But random sequences can be observed and studied, too. In sum, the observation of politicians brings with it a sharpened sensitivity to *sequence* as a variable in political analysis.

Observation in the constituency also drives home the importance of sequence. My first encounter with each senator took place in the middle of an election campaign. To observe a campaign is to observe constant change. Contributions, poll results, organizational arrangements, staff, schedules, opposition activities, expectations, media treatment, the candidate's mind—all these ingredients change. The campaign you see in January is not the same campaign you see in the summer, or after Labor Day, or on November 1. Indeed, the campaign you see on the day you arrive is often not the campaign you see on the day you leave. And that is exactly the way the campaigners themselves perceive it. They talk to you about the campaign in the language of time, and of change and of sequence. They speak of stages and phases, of rhythms and flows, of plans and turning points, of gains and losses, of momentum and pace, of beginning games and end games. Campaigns, for all their improvisation, exhibit a good deal of sequential regularity. At the internal planning levels, there is a sequence involving anticipation, organization, and implementation. At the external, public reaction level there is a sequence involving credibility, expectations, and momentum. These sequences can be studied; and so can the random shocks that often alter them. Again, the campaign sequence is only a convenient example.

If, then, the activity of observing politicians has any "value added," it begins with the special sensitivity observation brings to context and to sequence. Politics is a contextual activity and a sequential activity. Students of politics must be students of context and of sequence. Students of American legislative politics need no reminders of the importance of context or sequence—much less of the importance of constituencies and campaigns. To some degree, then, the large lessons to be drawn from observation will simply reenforce ongoing research, and my remarks will add to the chorus. That is fine. For I do not wish to add to the number of variables we treat; I only wish to argue for the richest possible understanding of some we already recognize as central. Here I would like to push a little. For it does not follow from the recognition that context and sequence are important that they will be given due weight in our research. And it is precisely in the matter of due weight that the observational perspective can be most helpful.

Consider the constituency as context. Its importance for legislative politics is taken to be fundamental and indisputable. One would be hard-put to find a study in the entire field that does not include some mention of the constituency. Yet one would be equally hard-put to find a single constituency anywhere in the United States whose complexity in terms of "this politician in this context" and "at this time" has been analyzed by a political scientist. We love our constituencies, but we do not study them—not up close, in detail, and over time. Can we be satisfied that we know enough about the process by which politicians get recruited and then accumulate (or dissipate) name recognition, reputation, and trust, bit by bit, in multiple intraconstituency contexts over time, when we have yet to study that process for any legislator?

The same can be said for campaigns as sequence. We generalize about them often. But one would be hard-put to find any legislator's campaign for which its yearly, monthly, weekly, even daily sequences have been analyzed up close, in detail, and over time. Can we be satisfied that we know enough about the ways in which strategic options open up and close down over the course of the campaign, or about the kinds of choices that lead a campaign down one path rather than another, or about the choice points at which such branching decisions get made, when we have yet to follow a single campaign from start to finish? With so little closely detailed investigation of so common a context as the constituency and so common a sequence as a campaign, it seems unlikely that we have yet given these variables the close, hard study they deserve.

From an observational perspective, then, there remains plenty of room for some finely grained, finely calibrated studies of context and of sequence. The observational mode is well suited to such studies. And, indeed, that is the direction in which the close personal observation of politicians will inevitably take us. It is a direction in which we could usefully go. In generalizing about the actions and interpretations of our politicians, we must specify, with the greatest care, the conditions under which each generalization holds. Context and sequence are two such conditions. Perhaps we can draw precise specifications of these conditions without undertaking fairly microscopic analyses of them. But I doubt it.

I would extend the argument to the other master context in which all legislative politicians work. That is the capital city, where the central activity is governing. Students of legislative politics have given a great deal of attention to the activity of governing and to its sequential patterns, fixed as they are by the formal rules of the legislative process. Here, in our case studies of "how a bill becomes a law," can be found the most numerous examples of scholarly observation. And they have been of enormous value to us. But I should like to push a little here, too. A careful detailing of the perceptions, interpretations, and actions of legislative politicians, developed from the closest possible vantage point, and undertaken with a special sensitivity to context and sequence, can yet improve our understanding of what politicians do in the legislature and why. I shall try to support this argument for "value added" with an extended example.

Ronald Reagan's first foreign policy dispute with the Senate arose over his proposed sale of AWACS planes to Saudi Arabia in the fall of 1981. As governing sequences go, the AWACS controversy was well bounded. By law, the arms sale proposal—the largest in U.S. history—had to be submitted to Congress, where it became subject to a congressional veto if majorities of both houses voted, within 30 days, to disapprove it. The president formally submitted it on October 1 and it was voted on—up or down—by the Senate on October 28. As governing sequences go, also, the AWACS controversy was well publicized. Media interest grew exponentially as the sequence came to its end in the Senate. The House had already voted overwhelmingly to disapprove the sale; the president's prestige and power were thought to be hanging in the balance; the outcome was unpredictable almost until the vote itself. When the vote occurred, every senator was in his or her seat, and every senator voted. It was, thus, a sequence that was easy to follow; and it was a vote about which every senator would have something to say.

Since each of my senators was involved —if only to cast a vote—I talked with them and their staffs about what they saw, what they did, and why. If we think about them in terms of context and sequence, they cluster in three groupings, each group deciding in a different context and at a different point in time. I call them the early deciders, the active players, and the late deciders. I shall discuss two senators from each grouping. And, with deep apologies (to you not them) I shall designate them by letter.

Two early deciders, both Democrats, were opposed to the sale. They saw themselves, quite simply, as supporters of Israel and of its position. Accordingly, both committed themselves to that position months before the vote took place. "Basically," said the foreign policy staffer for Senator A, "we were going along with the Israeli position from the beginning. . . . We signed everything that came along. . . . Whatever the argument was, we signed on." Because he was a member of the committee of jurisdiction, Senator A was attentive to the issue from its inception. As soon as the administration floated the idea in February, he joined seven committee colleagues in signing a letter to the president expressing "serious" and "deep" concern about the idea and asking the administration to consult with the committee before taking action. In June he signed a letter circulated by Senator Robert Packwood asking the president not to submit the sale to Congress. And in September he signed Senator Packwood's official resolution of disapproval, on which the eventual vote would be taken.

Viewed from a distance and in the abstract, Senator A expressed an early, strong policy preference and he voted it. Nothing about his behavior seems problematical. But up close and in context there are complexities of choice to be observed. Despite his normal activism on foreign policy issues, he made a deliberate decision not to extend his influence beyond the acts of signing and voting in this case. "It was not something we focused on," said his foreign policy aide, "We weren't involved. . . . We never took an active part in it. The most active we ever got was the senator's statement in committee when the vote came up there. . . . I wouldn't rank it as one of the biggies we've handled." Senator A did not focus on AWACS because he was preoccupied with other matters at that time. And we are reminded that a strong policy preference does not translate automatically into an equal measure of attention or of involvement.

The timing of Senator A's signature on the formal resolution of disapproval necessitated yet another decision. "We were undecided about signing," explained his staffer, "till late in the summer. Packwood was looking for sponsors even before the hearings were held in committee. [The committee chairman] wanted the members to hold off until they heard the arguments in the committee. We did—until Packwood had gotten into the 40s somewhere, looking for the magic 50 signatures. We didn't want to get in too late, since we had no doubt we'd get in eventually. I remember we discussed it riding in the car. The Senator said 'I don't want to be a Johnny-come-lately.' So we jumped in." And Senator A became number 47 out of the eventual 50 cosponsors. The content of his decision was never in doubt. But the timing of his decision was calculated so as to make certain he would receive full credit for that decision. He believed he would have lost credit with other supporters of Israel if a 50-car pro-Israel train had left the station without him. Whatever the strength or the content of his policy preferences, therefore, he still had to decide how and when those preferences should be publicly expressed. Decisions about how to vote are separable from decisions about attentiveness, decisions about involvement, and decisions about timing. We have devoted far more energy researching the first kind of decision than the other three. Observation could stimulate a useful corrective.

Senator B also committed early and did not change. Like Senator A, he contributed no more to the process or the outcome than to sign and to vote. Senator B became attentive to the AWACS issue not because of his committee, but because of an early spring trip to the Middle East. "Not long after my trip," he said, "someone at home asked me what I thought about AWACS. I said I was against it." Like Senator A, he interpreted the issue as one of support for Israel; and he made his decision in those terms. In his post-trip interview in April with his state's largest paper he said, "This vote could be the most important vote this session, next to the economy. It's a symbolic vote. Where do you stand, with the Arabs or the Jews?"

He was quick to sign both the June Packwood letter and the September Packwood resolution of disapproval. "I was one of the early signers of the Packwood letter," he explained. "I thought that since I had declared a position on the issue, I would become very conspicuous if I did not become a part of the Packwood group." It was the fact of his early commitment more than his policy preference that motivated these actions. And it continued to do so. "I can't say I didn't have second thoughts about it. . . . I never had that anti-Saudi feeling that Packwood had. I'm not sure why I committed so early. But once I did, I felt I should stay to the end." Near the end, the president invited Senator B to the White House for a chat. He went. "They must have thought they saw a soft underbelly," he said, "that they would say, 'Senator, you've just got to support the president.' One of the deficits a politician can get is to be known as a flip-flopper. So I said to myself, You'll just have to ride this one out." Which he did. "I told the president that I would shed no tears if he won, but that I could not change."

Once again, at a distance and in the abstract, Senator B's behavior is quite straightforward. He expressed a policy preference early and strongly, and he voted it. But observation up close and in sequence reveals something more complex. The strength of his commitment bore little relationship to the strength of his policy preference. And his vote can be explained more by the timing of his commitment than by the substance of it. He committed early; and that act constrained every subsequent interpretation and action. He deemed constancy of commitment to be crucial to his success as a politician. And he interpreted the two Packwood initiatives and the presidential chat as situations in which that connection was being tested. The cases of Senators A and B suggest that the more carefully we study sequence, the more carefully we shall have to study timing. If we are to explain outcomes, who decides when may be as important to know as who decides what. We have devoted more energy to studying policy positioning in space than to studying policy sequencing in time. To our rich comprehension of the politics of left, right, and center, we can usefully add an equally rich comprehension of the politics of early, later, and late. Here, too, observation can stimulate a useful corrective.

The cases of A and B suggest, also, that we must think of any politician's goals as mixed, and subject to change. As contexts change and as sequences unfold, the relative importance of goals may change. Senator A's policy goal came to be tempered by that of political success. And Senator B's policy goal gradually gave way to one of political success. Both senators were cultivating professional reputations at home and in Washington. Each acted so as to protect that reputation —A so as not to be known as a "Johnny-come-lately," B so as not to be known as a "flip-flopper." These interpretations and actions suggest the supreme importance politicians attach to their reputations. They suggest, too, that the cultivation and protection of favorable reputations involve favorable balances of credit and blame. They suggest, finally, that timing, within a sequence of activity, can be a key determinant of credit and blame. The pursuit of favorable reputations and favorable credit/blame balances can usually be studied by observation.

Senators C and D, two active players, interpreted and acted very differently from Senators A and B. First, they deliberately decided to exert an influence on the outcome beyond their signatures and votes. Second, while both thought of themselves as staunch supporters of Israel, neither interpreted the issue in simple "for or against Israel" terms. Third, because they wanted to maintain negotiating flexibility, they did not commit themselves until much later than A or B. Fourth, by postponing their commitments, they became active in an altered context. In the spring there was much position taking, as with A and B, but no negotiating. In the late summer and early fall, Washington wisdom held that the Senate would disapprove the sale; and that expectation created a context in which the administration had incentives to negotiate—which they did, first with Senator C, later with Senator D.

Senator C, a Democrat, focused on AWACS because, as a member of the committee of jurisdiction, he had long been involved in the problem of arms sales. He possessed a widely acknowledged expertise and commanded much collegial respect in that field. He had developed strong policy preferences. During most of his involvement, these policy goals guided his actions. His pivotal concern was that the AWACS technology not fall into unfriendly hands. His solution was to have the planes flown by joint U.S.-Saudi Arabian crews, and he worked to negotiate a change along these lines prior to the administration's formal submittal. His proposals attracted the support of other senators.

He described his influence in the language of sequence:

> I was involved in it all the way through, but my maximum involvement came early. I was light years ahead of anyone in the Senate when AWACS came up. . . . From the beginning, I understood what the problem was and I had strong views about it. I believed that wherever that plane went—that very sophisticated piece of technology—our people should go too. I'd say 25 to 28 senators came to me early on to ask for my views. I sat down with each of them for half an hour and talked to them in detail. At this time, I think I had quite a bit of influence on a lot of people's thinking.

His influence came from his ability to redefine the issue and, for a time, to persuade others to focus on his definition of the issue—in this case, the willingness of the Saudis to accept joint Saudi-U.S. crews on the planes. In mid-September Senator C participated, for a few days, in intensive conversations with Senate leaders, with administration officials and Saudi representatives. At one point he thought he had brokered, at a meeting in his office, an agreement on joint crewing. But the Saudi government said it would not accept any such arrangement; the administration decided not to push the Saudis, and two days later the proposed sale was formally submitted to the Senate unchanged. "We had an agreement we thought would save the world," said tor C, "but it turned to worms overnight." And the context of negotiation changed accordingly.

Senator C quickly broke off his negotiating relationship with the White House—largely on the grounds that the Saudi-U.S. actions had rendered his policy goals unattainable. The administration asked him to continue negotiating. But with the demise of his most preferred policy, another personal goal entered his calculation. Senator C wanted to be president of the United States. He feared that any future negotiations would be conducted on the administration's terms, and that he might be "set up" by them to get none of the credit and much of the blame if the sale went through—blame from American Jews that no Democratic hopeful could withstand. As in the earlier cases, changing contexts changed the mix of goals; and strategic calculations were heavily weighted with expectations concerning credit and blame.

When Senator C broke off negotiations, other negotiators entered the picture. Senator D was one of them. And he saw the world very differently from Senator C. Senator D was a Republican. He was a first-year senator. He had no recognized expertise. He had never visited the Middle East. He was not a member of the committee of jurisdiction. He did not have strong policy preferences. But he had a high priority goal. "I want to be known as an effective senator," he said. To that end, he wanted to start, as he put it, "getting involved in the issues, taking some initiatives, doing the work and getting things accomplished." And he had a lot of energy. AWACS became his proving ground. "[It] took more time than anything else I did this year," he said later. He focused on AWACS partly because "I've always been interested in foreign policy," but mostly because "I like the president and I want him to succeed. This is the president's first foreign policy vote. We don't want him to lose it." A Reagan loyalist, with no presidential ambitions of his own, he was not constrained to negotiate at arm's length like Senator C. And he plunged in.

"When I first read about AWACS," he said, "I was lukewarm toward it. . . . But I sat here thinking. There must be some way to get this through. . . . I even went so far as to call my friend George Will, and I asked him, 'George, how do we get the president out of this mess?' " The question at issue was being reinterpreted for yet a third time—from "Are you for or against Israel?" to "How can we get the Saudis to make concessions?" to "How do we get the president out of this mess?" For my senators, at least, this sequence of interpretations ran parallel to a sequence of action.

Senator D went to majority leader Howard Baker and offered to sound out the uncommitted freshman Republicans on Senator C's joint crewing idea. Senator D himself favored some variant on that idea, and he became Baker's liaison with half a dozen freshmen. When the joint crewing notion "turned to worms," Senator D and his first-year colleagues put forward the idea of an informal letter from the president promising adequate safeguards for the technology—a so-called "letter of certification." Senator D drafted that letter, and others in his group worked it over. Five days after the president sent up his proposal, the freshman group met with James Baker in Senator D's office. "We told Baker," he said, "that this was what we needed if we were to go along, that otherwise we would vote against the sale." The negotiation produced "95% agreement" on the letter. And the letter, said Senator D, "became the mechanism for winning over doubting senators. Each one would read it, change it a little bit, and put his Hancock on it somewhere." The letter, as signed by the president, enabled Senator D to make a firm commitment and go home to explain it in mid-October. He thinks the letter provided a necessary rationale for three others in his group. So, Senator D got involved and had an impact on the outcome. It was not large, but it was timely and tangible—and it moved him a measurable distance toward his personal reputational goal.

Legislative outcomes, we know, are invariably negotiated outcomes. This brief and episodic observation of a negotiating sequence suggests that negotiations are, like campaigns, a constantly moving stream. The context in which Senator C's early negotiating efforts took place was not the same context in which Senator D's later negotiating efforts took place. With the passage of time comes contextual change. And with contextual change come new negotiators pursuing different goals, redefining the issue at hand, altering the decision context for others, and, altogether, shaping new coalitional possibilities. For purposes of coalition building, the United States Senate of late September was simply not the same United States Senate that existed in mid-October. That sense of constant motion ought to superintend all our studies of governing. It is a sense that can be sharpened and shaped by observation.

Also the goals of Senators C and D remind us that for every politician, all governing sequences, like all campaign sequences, are embedded within a yet more encompassing sequence. That is the individual's career sequence. Whenever you observe a politician, he or she is at some stage in a career that stretches back in time and reaches forward in time. His behavior can be interpreted from the perspective of the career path that brought him to where he is, and of the career path he expects will take him where he wants to go. In the case of C and D, their long-run aspirations help us understand their short-run interpretations and actions on AWACS: Senator C's desire to be president; Senator D's desire to be an effective senator. Career aspirations, career building, and career sequences are highlighted by observation and can usefully be studied by observation.

Senators E and F, two late deciders, were among the very last members to make a final decision. Senator E decided the night before the vote. Senator F decided the afternoon of the vote. Like the two early deciders, they had no extra involvement in the process; like the early deciders, they signed both Packwood initiatives. But unlike the early deciders, E and F changed their position and voted for the sale. They did so in yet another decision context at yet another stage in the decision-making sequence. In late October, President Reagan and the presidentially oriented media entered the picture, and together they reinterpreted the issue at hand. An issue revolving about the various conditions of the sale gave way to one concerning the president's ability to conduct foreign policy. As Senator B, reflecting on his presidential chat, put it, "The substantive issue was lost sight of. The question became, 'Are you for or against the President?' " In this latest of interpretive contexts, Republican Senators E and F reconsidered their earlier decision.

For Senator E, it was the media—aided by the president—which refocused his attention on AWACS. "The whole issue changed in the last week or 10 days," he said. "The media began to play it up as a question involving the president's ability to conduct foreign policy. . . . The media hyped the deal way out of proportion to what it really deserved. But the media hype made you reevaluate your vote. Had it not been for the media hype, the issue would have been strictly the arms sales. And I would have been very comfortable voting against AWACS." His policy preference never changed; it simply became irrelevant to a vote cast on institutional grounds. "It was," he said, "something I felt changing gradually. No one thing brought me to my conclusion." Not even a last minute chat with the president. "I had a very pleasant conversation with the president. . . . At the end I said, 'Mr. President . . . I wish I could say I'm with you, but I can't. All I can say is, I'll keep thinking about it.' " The night before the vote he had a two-hour talk with "the political advisor in the family"—his wife. "We made a checklist—zip, zip, zip on one side, zip, zip, zip on the other side. She asked me 'Well, what do you think it adds up to?' And I said, 'It looks like I ought to support the president.' " And he went to the phone to tell Senator Packwood that he could no longer support the disapproval resolution.

Senator E's early commitment was not controlling for him, as it was for Senator B, because, unlike Senator B, he did not believe that his reputation was at stake. With his Senate colleagues, Senator E's reputation was undamaged because his change of position moved him back under the protective cover of his party's majority and of his party's leader. With his constituents, Senator E simply did not believe that his reputation depended on constancy of commitment. "The state media were very good to me," he said afterward. "With them I have established a reputation as someone who is independent, who does as he goddamned pleases, who can't be pushed around. That helps. It gives you room to cast a vote like this without people saying you've been had. It's not easy to climb down from your position and take another one in the full glare of national publicity. But the state media treated me like I knew what I was doing." While all politicians act so as to cultivate and protect their professional reputations, how they go about it depends not only on the reputation they want, but on what they think their reputation already is. That crucial matter of perception and interpretation can be studied by observation.

There are nearly as many reasons why senators pay attention to a problem as there are senators. It was committee membership for A, a trip for B, expertise for C, a desire to help the president for D, the media for E. We need to know what these stimuli are if we are to understand the subsequent involvement, interpretation, and action of each individual politician. For Senator F, conversations with Israeli friends triggered his reevaluation. He thought long and hard about the problem, and the immense pressures he felt on the day of the debate were self-generated.

He had a strong attachment to Israel; but he gradually became convinced that Israel might be just as well off with the sale as without it. "The more I looked at it," he said, "the more I began to worry about what the situation would look like if the deal did *not* go through—the Saudis with their hurt pride, the British in there selling the Saudis the same weapons, Israel asking us for arms to control the Saudis, Israel blamed for the defeat of the sale, and a president who had his wings clipped in his first foreign policy effort. As bad as the sale was for Israel, I began to think the defeat of it might be worse." He found that most of his Israeli friends agreed with him. He did not reinterpret the question in institutional, "for or against the president" terms as E had done. Senator F's preferred policy changed because he took in more information relevant to his original question—the effect on Israel. His preference changed because his information had changed. Either way, through the imposition of a new issue or through the addition of information on the old issue, preferences can change as the governing sequence unfolds. Thus we are reminded that the sequence itself has an autonomous effect on the politicians involved in it. And it must be studied as such. Observation at close range is well suited to studying this impact of processes on outcomes.

On the eve of the vote, Senator F was leaning but undecided. "I stayed up all night before writing a pro-AWACS speech. But I still was not certain how I was going to vote. . . . Up until 10 minutes before I gave my speech I honestly wasn't certain what I was going to do." His course of action was finally fixed by a confluence of context and of sequence. He found himself in a situation in which he could cast the deciding vote for the sale, and he consciously did so. "A little after noon," he said, "I went to Howard Baker and I asked him what his count was. He said, 'Forty-nine.' I asked, 'What about X?' He said, 'He won't talk to anyone.' I asked, 'How about Y?' He said, 'We can't get to him.' " "I was the only one they could count on," explained F. And he added, "If the sale was going through, and if I could be the fiftieth vote, then I could exert more leverage with the administration and be in a better position to help Israel. . . . Conversely, I didn't want some oil state senator to be the fiftieth vote." The sale went through, 52 to 48.

On his credit/blame balance sheet, Senator F got the worst of both worlds. Supporters of Israel assumed his was the decisive vote; and they blamed him for their loss. "I got more flak from that vote by far than any I have ever cast in the Senate," he said later. And supporters of AWACS gave him no credit for their victory. Three years afterward he said, "Do you know, the administration never thanked me for helping them on that vote, never gave me anything, never acknowledged that I helped them." So much for our abstract generalizations about the special benefits accruing to the latest deciding, pivotal voters. In the concrete, it seems, there are interpretive contingencies to be reckoned with. You are pivotal only if the distributors of credit and blame think you are, and politicians understand that condition. Thus, the allocation, as well as the creation, of credit and blame should be studied—and they can both be studied by observation.

It should not be thought that I have told *the* story of AWACS. Far from it. At best

I have told six little stories to feed into the larger story of AWACS. But the six stories suggest how complex *the* story of any large legislative decision must be, and how many different decision contexts and decision sequences are involved. They further suggest how much research room yet remains for the microscopic, observation-based analyses of the governing activity of legislative politicians. If, that is, such analyses have value. I have tried to make the case, by example, that they do.

The stories of decision making by six senators have been told to suggest that observation can be an aid to discovery, description, and theorizing. They have been told to further suggest that this "value added" flows from a heightened sensitivity to context and to sequence. In these briefest of discussions—about such analytic foci as attentiveness, involvement, and timing in decision making; or about changing interpretive contexts and changing contextual impacts on decision making; or about calculations concerning goals, reputations, and credit/blame balances; or about interpretive sequences, negotiating sequences, voting sequences, and career sequences—there is, I would argue, sufficient warrant for the claims of observation. These claims have been stated in incremental language, as befits the notion of "value added," to wit: "sensitize," "emphasize," "suggest," "remind," "correct," "stimulate," "aid," "improve," "highlight." But in research involving politicians, these modest claims may represent an indispensable increment of knowledge and understanding.

I shall close by raising two questions about observation. Do we need political scientists to do it? And if we do, are enough political scientists doing it?

As for the first question, there is a huge corps of journalists who observe politics every day. They do it very well. And we are already heavily dependent on what they tell us about our politicians. If we do not do it, they will do it for us. But it is in the nature of their occupation that they have neither the training, nor the patience, nor the interest to conduct a dialogue with political science theorizers. Journalists are not conceptualizers or generalizers. They are more interested in episodes than regularities. Their observations are not driven by the questions of political science. Put it this way, if my remarks this evening are interchangeable with the remarks a journalist would have made to you, then I shall desist. If not, I shall argue that we cannot leave the field to the journalists, that we need political scientists to go take a first-hand look at our politicians and report back to us. And we need political scientists to keep doing it. For only we can persist in attaching observation to theory. Surely we cannot construct theories of politics on the basis of observation. But we might ask ourselves whether it is possible to construct theories of politics without observational perspectives—if, that is, we wish our theories to encompass the serious activity of our politicians.

As for the second question, I believe that not enough political scientists are presently engaged in observation. Unless I misread our journals, our graduate methods courses, and our reward structure in general, young political scientists have little incentive to expend time and energy observing politicians. Observation-based research is a rarity in the *American Political Science Review.* There are many reasons for this; and each of them may be valid in its own terms. But the question is a disciplinary one. How much value do we place on observation as a research mode? How much legitimacy do we wish to bestow on observation-based research? Have we tried our very best to teach observation and failed? Or have we made a standing decision against its claims of added value to us? We shall not answer these questions until we examine them with care. Perhaps, sometime and somehow, that careful examination will take place within the political science profession. I hope so.

Note

This essay is the presidential address presented on August 29, 1985 at the 81st annual meeting of the American Political Science Association in New Orleans. I owe a very large debt to many of my fellow political scientists, upon whose work I have drawn. But this essay, printed here exactly as it was delivered, was prepared as a public talk, so it does not include footnote references to the work of the very large number of political scientists to whom it is indebted.

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AMERICAN VOTER TURNOUT IN COMPARATIVE PERSPECTIVE

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Despite relatively favorable citizen attitudes, voter turnout in American national elections is far below the average of 80% of the eligible electorate that votes in other industrialized democracies. The American institutional setting—particularly the party system and the registration laws—severely inhibits voter turnout, and probably also accounts for the unusual degree to which education and other socioeconomic resources are directly linked to voting participation in the United States.

Using a combination of aggregate and comparative survey data, the present analysis suggests that in comparative perspective, turnout in the United States is advantaged about 5% by political attitudes, but disadvantaged 13% by the party system and institutional factors, and up to 14% by the registration laws. The experience of other democracies suggests that encouraging voter participation would contribute to channeling discontent through the electoral process. Even a significantly expanded American electorate would be more interested and involved in political activity than are present voters in most other democracies.

Seen in comparative perspective, American voter turnout presents an interesting paradox. Americans seem to be more politically aware and involved than citizens in any other democracy, yet the levels of voter turnout in the United States are consistently far below the democratic average. The resolution of the paradox lies, apparently, in the nature of voting as a form of participation.

In their study of different forms of political participation in seven nations, Verba, Nie, and Kim (1978) suggest a distinction between two types of forces that shape political activity. On one hand, there are the attitudes and characteristics that individuals bring to the participatory arena. Participation is, in general, facilitated by greater socioeconomic resources and by general levels of political awareness and self-confidence. On the other hand, participation is also facilitated or hindered by the institutional context within which individuals act. Legal rules, social and political structures, and configurations of partisanship all present the individual with conditions that shape his or her choices, and are relatively difficult for the individual to change. Analysis of

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

the different types of participation suggests that voting is particularly likely to be dominated by institutional factors.

The present analysis attempts to explain the paradox of American voter participation in terms of the conjunction of the two types of forces cited above. Americans do possess political attitudes that encourage their voting activity. If citizens in other democracies possessed the American configuration of attitudes, their voter participation would on average increase. However, the American attitudinal advantage is only a marginal enhancer of voting. Its effects are limited, first, because in recent years the American attitudinal advantage has declined, and, more importantly, because voting is so powerfully shaped by institutional context. In comparative perspective, the American registration rules, electoral system, and party system inhibit voter participation, outweighing by far the attitudinal advantage.

The subsequent discussion is divided into three sections. The first of these presents aggregate evidence on American advantages in social and attitudinal resources compared with other modern democracies, and contrasts these with the putatively disadvantageous institutional conditions. The second section uses econometric analyses of survey and aggregate data to estimate the relative importance of specific individual and system characteristics that affect voter participation. The third section analyzes the contributions made by each type of factor to relative American voter turnout by combining the estimates of the impact of various individual and institutional attributes with the evidence on the relative American advantages and disadvantages.

The Cultural and Institutional Environment for Voting

Table 1 presents some evidence that allows us to characterize the American political cultural environment in comparative perspective. The table is divided into several sections. The first compares the democratic publics on some attitudes often demonstrated to be associated with political participation. The second section compares the citizens' reports of three forms of political activity that might be expected to be related to their likelihood of voting. The third section compares the electorates on some demographic characteristics expected to be related to voter turnout.

Although attitudes facilitating participation declined in the United States in the late 1960s (Abramson and Aldrich, 1982; Nie, Verba, and Petrocik, 1976), comparison with political attitudes in the European nations still shows the American public to good advantage. Partisanship has declined, but is still equal to or above average. Eighty-three percent of Americans named a party they usually felt close to, placing the United States behind only the Netherlands and Finland in a twelve-nation comparison. On the other side of the partisanship scale, 14% said they felt very close to their party, a figure slightly behind only two other countries (Austria and Italy), although a drop from the America of the early 1960s.

Despite the drop in political confidence, the United States still led all countries in the number of citizens believing that they had some say in government. In general political interest, the 90% of Americans who reported at least some interest were comparable to their counterparts a decade earlier, and well ahead of the cross-national average. Only in the decline of political trust did the Americans drop from a leading position to one well back. The United States ranked ahead of only Italy in trust of the national government in 1974. (While most of the other attitude levels stabilized in the mid-1970s, and did not decline further, trust actually dropped an additional 8% between 1974 and 1980.) But political trust, although related to voter participation in America, was the

Table 1. The Cultural Environment for Voter Participation: America Compared to Other Industrial Democracies in the 1970s

Variables		United States	Averages from Other Industrial Democracies		Rank of the United States
		%	11 Nations %	7 Nations %	
Attitudes Facilitating Voter Turnout					
Partisanship:	mention a party they usually feel close to	83	73	77	3/12
Efficacy:	reject: "People like me have no say in what the government does."	59	—	33	1/8
Trust:	trust the national government to do what is right most or all of the time.	34	—	47	7/8
Interest:	possess at least some interest in politics.	91	—	75	1/8
Other Forms of Political Activity					
Discussion:	discussed politics with others	89	68	74	1/12
Persuasion:	tried to convince others during elections.	40	—	27	2/8
Party Work:	worked for a party or candidate during an election.	30	—	14	1/8
Demographic Characteristics Facilitating Participation					
Education:	beyond lower (ninth grade) education.	79	—	39	1/8
Occupation:	white collar jobs in work force.	64	49	48	1/12
Age:	over age 34 among population of voting age.	64	66	66	11/12

Sources: See acknowledgement in Notes section for surveys. Occupation from Taylor and Jodice, 1983. Age from United Nations, 1979-1983. Other Democracies (Austria, Britain, Finland, Germany, Italy, Netherlands, Switzerland, Belgium, Denmark, France, and Ireland): Political Action Study (Barnes et al., 1979) and Euro-Barometer (Rabier and Inglehart, 1981).

least important of the four attitude variables in the American attitude studies.[1] We must conclude, therefore, that despite the decline in the period from 1960–1975, American political attitudes should still facilitate more political participation than political attitudes in other democracies.

This conclusion is strongly supported by the second section of the table, which compares three other measures of political activity: (1) discussing politics with other people, (2) trying to convince others to vote for a party or candidate during an election campaign, and (3) working for a party or candidate during elections. Nearly 90% of the American public reports discussing politics at least some of the time, compared to an average of only 68% across the 11 European nations. (Comparisons at the most active end of the discussion measure yield similar comparative results.) Thirty percent of the American citizens report having worked during a campaign at some time, more than double the average for seven European nations in which this question was asked. In reporting having tried to convince others during election campaigns, the Americans trail only the West Germans, with 40% affirming such activity, in comparison to an average of 27% in seven other countries. These American results parallel other studies of the American electorate quite closely. The strong relative position of the American public in attitudes that facilitate participation and in various measures of political activity other than voting also appears in other comparative studies, particularly in the five-nation Civic Culture study (Almond and Verba, 1963), and in the seven-nation Participation and Equality study (Verba et al., 1978).

The last section of Table 1 compares the American and European mass publics on three important demographic characteristics that have been shown to be related to political participation propensities. The American participation studies, as well as the comparative studies, have demonstrated that possession of greater social and economic resources, particularly higher levels of education, is associated with attitudes and behavior that facilitate participation. While these comparisons must not be taken completely literally, given differences in occupational structure and in educational quality and content, they help explain the relatively high levels of political awareness and involvement in the United States. The average educational level is much higher in the United States than in most of Western Europe (reflecting the much older American concern for mass education, as opposed to the European elite emphasis). The American citizen is also more likely to hold a white-collar or professional job than his European counterpart, although here the differences are not quite so marked, as all these nations are relatively economically developed.

Only in its age structure do the demographics of the American public tell against political participation. Many studies have shown that the youngest segment of the electorate, in general those under age 35, tends to participate less in most forms of political activity.[2] At the time of the 1970 census, the proportion of young voters in the American electorate was above average, although the differences were not very great. The gap increased notably by the 1980 censuses, due in part to reluctance in a few European nations to lower the voting age to 18, but primarily to the "bulge" of young people entering the American electorate in the late 1960s and the 1970s. This demographic change, as we shall discuss later, did increase the gap in turnout between the United States and the other democracies. As late as 1970, however, the age gap was slight.

The picture of American political attitudes and demographic characteristics that emerges from Table 1 leads us to expect high levels of American voter participation. However, the institutional factors facing the American voters are for the most part highly inhibiting, compared with those in the other industrial nations. The following discussion summarizes these differences from the American point of view. The full distribution for the 20 contemporary democracies is presented in Appendix 1.

As has been emphasized in previous comparative studies of voter participation (Crewe, 1981; Glass, Squire, and Wolfinger, 1984; Powell, 1980), the legal situation is of great importance. In some nations, legal sanctions are used to encourage voters to go to the polls. While the nature of the penalties and the level of enforcement varies within and across countries, there is no doubt that such sanctions tend to increase voter turnout. Such mandated voter participation has not been used in the United States, but is present in Australia, Belgium, Greece, and Italy, as well as in parts of Austria and Switzerland.

Moreover, the registration laws make

voting more difficult in the United States than in almost any other democracy. In 16 of the democracies examined, initiative for registering eligible voters is taken by the government in some fashion. In Australia and New Zealand the citizens must take the initiative; however, they are legally required to do so, and subject to fines or other penalties for failing to register. Of the 20 democracies outside the United States, only France leaves voter registration to voluntary initiative of citizens (Herman, 1976). In France citizens are required to register in their community and to obtain identification cards, which facilitates voter registration. In the United States registration is entirely the responsibility of the citizens and no set of requirements brings them to the registration site. Moreover, although the residency requirement in the United States has now been limited to 30 days before a federal election, only a handful of states with small populations have day-of-voting registration. Other states vary greatly in their registration hours and places, and in the degree to which these facilitate registration (see Rosenstone and Wolfinger, 1978), but all require the voter to re-register if he or she has changed residence since the last election. As the 1980 census showed that 47% of the population had moved in the past five years, it would seem that about half the eligible citizens must, in effect, make a double effort to vote in a presidential election: first the effort to register, then to vote.

A feature of the institutional context that has been the subject of much debate and analysis among American political scientists, but whose comparative implications are harder to measure and assess, is the competitive context. Intuitively, it would seem that in elections in which the outcome was expected to be close, citizens would feel more reason to participate and, perhaps more importantly, party organizations and activists would feel more incentive to get their voters to the polls.

In Appendix 1, and in the statistical analysis in the next section, two possible aspects of such competition are examined. First, we consider the frequency with which control of the national chief executive, by a party or coalition of parties, changed after an election in the twenty-year period from 1961 through 1980. By this measure, there is no doubt that the environment of the American presidential election was among the most competitive in any democracy. In the United States, party control of the presidency changed hands following four of the six elections between 1960 and 1980. We must note, however, that there were few changes in party control of the national legislature in the United States in this period, with the Democrats maintaining control of the House and losing control of the Senate only in 1980.

The second aspect of competitiveness concerns the possible influence of the electoral constituencies on competition in different parts of a country. The idea is simply that the electoral constituencies help determine whether parties and voters have equal incentive to get voters to the polls in all parts of the country, or whether there may be reason to neglect less evenly balanced regions in turning out the vote. Where the chief executive is chosen by simple majority or plurality vote, all regions should be equally important (e.g., France). In countries where the chief executive is chosen by the legislature, as in the various parliamentary systems, the question becomes the nature of the constituencies electing the legislators. With proportional representation from the nation as a whole or from large districts, parties have an incentive to mobilize everywhere. With single-member districts, some areas may be written off as hopeless.

Various American studies have found such effects in races for state legislative seats and governor (Caldeira and Patterson, 1982; Patterson and Caldeira, 1983; and references cited therein). Studies in Britain and Canada (Denver and Hands, 1975; Irvine, 1976) also suggest effects that dampen participation in some districts. In this intranational consideration of competitiveness, we would expect the American situation to be most like those in the single-member district countries, as the state acts as the electoral unit in the electoral college, and its electoral votes are delivered as a block, rather than proportionally. We must note, however, that American states shift support in quite volatile fashion. In 1972 about two-thirds of the states went for a candidate by a margin of over 20%, but in 1976 only 4% did so, while in 1980 20% did so.

Another important aspect of the partisan context is the linkage between political parties and social groups. For a variety of reasons, we expect voter turnout to be higher in countries having sharper partisan-group differentials in support. Partisan choice should seem simpler to the less involved; cues from the personal environment of the individual (friends, family, and co-workers) should be more consistent; party organizers can more easily identify their potential supporters in making appeals and in helping voters to the polls on election day. The last column in Appendix 1 shows a measure of the differential ties between voters' membership in social groups and their partisanship. In some countries, as in Sweden or the Netherlands, to know a voter's occupation or religion enables us to predict his or her voting preference to a very great degree. In Sweden in 1964, for example, about 84% of those with manual labor occupations supported the Social Democrats or the Communists, while only about 32% of the voters with white-collar or farm occupations did so, yielding a "class voting" index of 52. In the United States in the same year the manual labor support for the Democrats exceeded white-collar support by only about 17%. Indices of partisan support based on occupation, religion, and church attendance were calculated from surveys in 20 countries. Appendix 1 shows the highest of these indices.[3]

As shown in Appendix 1, the party-group support differentials in the United States were only about half as great as those in the average democracy. In fact, the United States had one of the lowest levels of party-group support of any modern democracy. As the bases of the old Roosevelt coalition continued to crumble during the last 20 years, the vote scores on party-group differences fell even faster than these numbers, based on party identification, would indicate (see Abramson, Aldrich, and Rohde, 1983).[4]

While the party-group linkage measure seems to tap an important feature of the party system for voting mobilization, it would be desirable to have explicit measures of the strength of comparative party organizations. We would expect dense, penetrative, nationally-oriented party organizations to be most effective in getting voters to the polls in national elections. Unfortunately, there seem to be no reliable quantitative studies of party organization strength across nations. The only comparative study that even attempts to describe party organization in these terms is Janda's work (1980), which relies on expert coding estimates for 12 of the nations being considered here. The time period was the early 1960s.

The Janda study results are suggestive, and can be summarized here briefly. They portray the American party system as slightly above average in the sheer magnitude and extensiveness of party organization, but highly decentralized and with very weak ties to other social organizations. The results regarding extensiveness are interesting, but most of the major parties scored close to the maximum on the measures reported. There was not much variation across nations on the variable;

the slight American advantage resulted from weak scores by some of the smaller parties in multiparty systems. I have not been able to discover comparative survey results on voters' reports of contacts by parties or other measures of comparative party effort.

The measures of centralization and organizational ties between parties and social organizations, called "penetration" in the Janda study, differentiate the democracies more sharply. The American parties are highly decentralized, especially in the selection of legislative candidates. On a combined measure of organizational structure, funding, and candidate selection, the United States ranks lowest of the 12 modern democracies in the degree of centralization. On the "penetration" measure, examining the ties between parties and such organizations as trade unions, religious bodies, and ethnic groups, the American parties ranked ahead of only the Irish parties. This measure seems to be the organizational counterpart of the party-group linkage measure based on partisan behavior, and empirically is closely related to it. We would expect that the lesser capacity of American parties to make use of other social organizations to spread their messages and to get voters to the polls would hinder their mobilization efforts.

We can summarize the description of the institutional environment for voting in the United States in comparison with other democracies by saying that it is the inverse of our description of the cultural environment. With few exceptions (age structure and political trust), the evidence on American political culture suggests that it should facilitate all kinds of individual political activity. With one exception (experience of party changes in national control of the executive), the institutional factors would seem to make the act of voting more difficult, and to impede the ability of parties and activists to mobilize supporters through appeals or through election day efforts to get them to the voting booth.

Estimating the Impact of Individual and Institutional Variables on Voter Turnout

While the evidence in Section I suggests reasons for America's exceptional pattern of an involved, but nonvoting citizenry, we need much more precise estimates of the relative importance of factors at the individual and system levels believed to affect voting. As it is likely that variables at both the individual and system levels are significant, the ideal analysis would consider both types of factors simultaneously. Unfortunately, we do not have comparable attitude surveys for half of the industrialized democracies. For the moment, therefore, we shall develop separate models of voter turnout. One of these models will be based on aggregate analysis of system-level variables, using the full set of 20 industrialized democracies. The second will be based on individual surveys from nine nations. In conclusion we shall consider on a preliminary basis the interaction of levels.

Comparisons of American Voter Turnout

The first step is to measure American voter turnout in comparative perspective. The problem is fraught with technical difficulties, but the overall situation is quite clear. In comparison with other democracies, the United States has relatively low participation of its citizens in major national elections. Average turnout in presidential elections in the United States as a percentage of the voting-age population was 54% in the period from 1972 to 1980. In the other 20 industrialized democracies the average turnout was 80%. American national voter participation exceeds only that of Switzerland. Among the nations that did not have compulsory voting, average voter turnout was 77% of the population of voting age, nearly 50% higher than turnout in the United States. This comparison is probably the most valid and reliable of those available. Detailed data are shown in Appendix 1. Similar comparisons may be found in Crewe (1981), Glass et al. (1984), and Powell (1980).

These comparisons rely on official reports of votes in the election that determines most directly the control of the chief executive. In the United States and France, these are presidential elections; in the other industrialized democracies, the most comparable elections are for the national legislature. The major problem in comparison is the denominator of the voting ratio: the eligible population. As turnout tends to be quite stable from election to election, the use of averages does not conceal many changes, but does help even out small errors in the census survey-based estimates of the voting age population at the time of the election. Appendix 3 discusses problems for comparison created by resident aliens in the population.

An important point to recognize about American voter turnout in comparative perspective is its close relation to voter registration. The United States is unique in the low registration rate of its population of voting age. Comparisons of turnout as percentages of either voting-age or registered populations lead to similar numbers in most countries, but radically different ones in the United States. In the United States perhaps two-thirds of eligible citizens are registered; of the other democracies, only in Switzerland are less than 90% of the citizens of voting age registered.

A final comparative point considers the time perspective. In most countries voter turnout changed little from the 1960s to the 1970s, remaining rather stable from election to election. In the United States, however, turnout dropped notably, for reasons various scholars have discussed elsewhere. These changes only served to widen the gap in electoral participation between the United States and the other democracies. They did not create the gap, nor, in fact, did the decline in American turnout even change its rank against any other country.

Aggregate-Level Explanations of Voter Turnout

I have collected data on most of the contextual and institutional factors described in the previous sections for the modern democracies. We can use these to attempt to explain differences in turnout across the democracies. As the institutional conditions do not change very often, it seems more appropriate to compare country averages than to enter individual elections as separate cases. Table 2, then, presents the model for voter turnout as a percentage of the population of voting age. Theoretically, we would expect that in the presence of compulsory voting, other factors encouraging participation have less effect. The countries where voting is compulsory are therefore deleted in the models in Table 2. (If we include them and a dummy variable for compulsory voting, the latter is powerful and significant, while the other aggregate coefficients are slightly depressed.)

The figures for the 1960s and 1970s are shown separately, as we hope to have more confidence in the results that are consistent at the two time periods. With a few minor exceptions, the independent variables are also measured separately at the two time points. Spain is not included in the first decade, as it did not become a democracy until the late 1970s. (Absence of party-group linkage data for Switzerland before 1972 and Israel in the 1970s forces us to use the same figure for both decades.) Switzerland is included as a dummy variable because of the unique

Table 2. Aggregate-Level Explanations of Voter Turnout: Predicting Average Turnout as Percent of the Population of Voting Age in Democracies Without Compulsory Voting

Predictor Variable	Regression Coefficient	Standard Error	Standardized Beta
Voter Turnout in the 1960s: 1960–1970			
Age: Percent over age 34	.01	.32	.00
Automatic Registration	3.70	2.51	.23[a]
Frequent Changes in Executive	.43	1.58	.03
Nationally Competitive Election Districts	1.89	1.45	.22[a]
Party-Group Linkage Index	.38	.15	.41[a]
United States Dummy	- 1.71	7.74	-.04
Switzerland Dummy	-30.11	5.61	-.75[a]
N=15 R^2=.90			
Voter Turnout in the 1970s: 1971–1980			
Age: Percent over age 34	.38	.28	.13[a]
Automatic Registration	.33	1.97	.02
Frequent Changes in Executive	1.26	1.23	.11
Nationally Competitive Election Districts	1.78	1.09	.18[a]
Party-Group Linkage Index	.45	.18	.33[a]
United States Dummy	-12.30	6.46	-.25[a]
Switzerland Dummy	-38.50	4.89	-.79[a]
N=17 R^2=.94			

Sources: Sources and data coding shown in Appendix 1.

[a]Significant at the .05 level.

nature of its collective executive and other special features of Swiss democracy.[5] A dummy variable is also included for the U.S., with its special registration conditions.

If we consider the general aggregate models emerging from Table 2, we see that they are reasonably stable and consistent across the time periods. (With few exceptions, they are also consistent with the analysis of voter turnout in 29 democracies reported in Powell, 1980, 1982.) The coefficients for intranationally competitive electoral laws and party-group linkages are rather similar in both time periods; party-group linkages are particularly strong and significant. The impact of changes in the chief executive is both weaker and less consistent. The unstandardized age coefficients are larger in the second decade, presumably reflecting the lowering of the voting age in most of the democracies in the 1970s, increasing the weight of an under-34 component. The dummy for Switzerland reflects the significantly lower turnout in the Swiss system in both decades, although the gap has increased over time. The presence of automatic registration faciitates turnout; its effects are stronger if we delete the U.S. dummy. (The increasingly negative U.S. dummy is discussed below.)

The implications of these data are that voter turnout in the United States is severely inhibited by its institutional context. The only feature of the institutional context where the United States seemed to enjoy a clear-cut advantage was in the frequent changes in chief executive—a variable that was insignificant in each decade in the aggregate analysis. (Considering the Swiss case as unique, rather than occupying the end of a continuum, in this regard.) The U.S. was disadvantaged by voluntary registration, unevenly competitive electoral districts, and very weak linkages (perceptual and organizational) between parties and social groups. The distance of 23 points between American party-group linkages and those in the average democracy would alone have been predicted to depress turnout by about 10%, based on the average coefficient in Table 2.

These models also work very similarly if we use turnout as a percent of the registered electorate as the dependent variable. The major difference is a reversal in sign of the automatic registration variable. That is, the presence of automatic registration facilitates voting participation of the age-eligible population, but leads to lower turnout among the registered. Such effects probably reflect the differing degrees of interest and partisanship required to enter the pool of the registered in the two kinds of registration situations. It is consistent with the well-known fact that turnout of registered voters is actually very high in the United States.

One variable not shown in the table is worthy of comment. While comparable education statistics were not available, I did attempt to use a measure of the percent of the labor force in white-collar occupations to get at the greater socioeconomic resources and skills available in some populations. The white-collar variable was positively related to aggregate levels of political discussion, in the 14 nations for which I had such a measure, at a significant and positive level ($r = .53$), just as we would predict from participation theory and from the individual models examined in the next section. However, the percent white-collar was negatively related to voter turnout ($r = -.18$), and when entered into the multiple regression with the institutional variables tended to be reduced toward zero. Accordingly, the white-collar variable is deleted in Table 2; its effects in mobilizing awareness are not sufficient to make an impact on turnout in the aggregate data analysis.

The analysis in Table 2 makes it possible to estimate the effects of the institutional variables in shaping voter turnout in modern democracies. I would emphasize, however, that we must be careful in the substantive interpretation of the institutional variables. The variables for intranational constituencies that enhance competition, and those for party-group linkage are the major source of concern. They are getting at some institutional property of the systems that affect turnout, to be sure. But these two variables are themselves related, and, in the subset of 12 countries for which we have the rather doubtful data, both are related to party organizational structure—especially party penetration of social groups. The party organization variables are not entered in the models, because of the limited number of cases and dubious nature of the data. If we do analyze those 12 countries, we find that centralization and penetration of social groups are strongly associated with turnout and with party-group linkage. Extensiveness, which has little variance, is not related to turnout. In multiple regression analysis, however, party-group linkage tends to reduce the impact of centralization and penetration to insignificance. Given the measurement problems, it is unwise to be confident about which of these aspects of the party and electoral system are the ones shaping turnout.

Individual-Level Explanations of Voter Turnout

Our analysis of voter turnout at the individual level relies on comparative survey studies. We would like to know two things. First, are the processes of voter involvement in the United States similar to those in other countries despite the differences in context? Second, if not, are there reasonably similar processes operating in other democracies, so that we could estimate the relative importance of various individual-level variables if the United States did have electoral and party contexts more comparable to those in other countries?

One difficulty should be noted first. We know that American survey studies consistently report greater levels of participation than do aggregate statistics. The difference reflects in part the population reached by the completed survey, and in part a tendency of citizens to overreport their participation. Judging by analysis of the voter validation studies conducted by the University of Michigan Survey Research Center (SRC) in 1964, 1976, and 1980, the overstatement of participation of some 18% in the 1970s reflects about an 11% net overreporting by citizens, and a problem of about 7% in the survey reaching parts of the population of voting age (Clausen, 1968–69; Katosh and Traugott, 1981). Citizen surveys in most other modern democracies do not show as large an overstatement as do the American studies. However, the percentage of nonvoters who report voting in other countries seems not dissimilar to that in the United States—about one-fourth to one-third the nonvoters in the sample. (This statement is an informed guess, based on comparison of official and reported turnout across 14 surveys; the proportions due to sampling and response errors cannot be estimated accurately.) It is reassuring for our purposes that the relative rank of the United States among the nations which we shall be comparing here is the same using either survey or official statistics, with the United States ahead of Switzerland, but behind the other nations.

Table 3 uses standard demographic variables plus party identification (ID) to predict political discussion and voting in the most recent national election from surveys in nine nations, including the United States. The left side of the table shows the model for voter turnout; the right side shows a model for political discussion. On the far left side of the table we see categories of increasing education and age, as well as entries for sex and party identification. The cell entries show the increased probability of voting as we move, for example, from one education group to the next, in comparison with the base-line group. In the United States there is on average no increased probability of voting as we move from sixth grade to ninth grade education, but an increase of 10% as we move from ninth grade to the eleventh grade, and another 17% for actually completing high school. Turnout among the college educated is 35% greater than among those with a primary education only. We can compare these sharp education effects with the average effects in the other eight nations: a consistent, but small, increase of 2–3% as we move up the comparable categories, with turnout among the college educated only 10% higher than among those with a primary education. The American voter participation process is obviously far more affected by education levels than the process in other nations.

Table 3, and Table 4 below, show the unstandardized regression coefficients (multiplied by 100) from the ordinary least squares regression equations that predict voting using demographic and attitudinal variables. The dummy variables for each group category allow us to make direct comparisons with the base-line category and observe possible curvilinear effects, while controlling for other variables in the equations. Because voting is dichotomous, however, there is a potential problem of misestimating the magnitude of the coefficients by using multiple regression. The use of LOGIT or PROBIT provides reliable estimates with such dichotomous dependent variables. All the equations in the individual level analysis have been reestimated using LOGIT and PROBIT. In fact, as usually

Table 3. Individual-Level Explanations of Voter Turnout: Predicting the Increase in Probability of Voting and Talking Politics in Nine Nations

	Predicted Increment in Activity Relative to Base-line Group in the Category[a]			
	Percent Who Voted in Last National Election		Percent Who Discussed Politics	
Independent Variables	United States	Eight-Nation Average	United States	Eight-Nation Average
Education Level[b]				
Basic	—	—	—	—
Lower	(0)	2	10	11
Extended Lower	10	4	16	17
Middle	27	7	24	25
Post-Secondary	35	10	29	28
Sex				
Male	—	—	—	—
Female	- 6	- 1	- 5	-12
Age				
20–25	—	—	—	—
26–29	9	21	(5)	(1)
30–39	21	25	(5)	(0)
40–49	25	30	(3)	(- 1)
50–59	32	(30)	(5)	(- 2)
60+	40	(30)	(- 2)	- 8
Party Identification				
No	—	—	—	—
Yes	18	17	13	13

[a] These numbers are 100 × the unstandardized regression coefficients in regression equations with dummy variables for each group except the base-line group in each category. Coefficients in parentheses are not significant at the .05 level. As demonstrated in Appendix 2, LOGIT and PROBIT models provide nearly identical estimates. The eight nations are Britain, West Germany, Netherlands, Austria, Italy, Switzerland, Finland, and Canada. (Coefficients are averaged, not taken from a single pooled data equation.) For Canada the analysis uses federal party identification in 1974 and the same education categories that were used in the United States.

[b] The education variable is based on codes constructed for each country that attempt to identify comparable levels across countries. See Barnes et al. (1979), pp. 584–588. In the U.S. the levels correspond to the following categories: Under 7 grades completed, 7–9 grades, 10–11 grades, Completed High School, and Post High School.

seems to be the case, the solutions are virtually identical for those with ordinary multiple regression. Appendix 2 demonstrates the extremely similar predictions from the three models. As multiple regression provides more readily interpreted coefficients, it is used in Tables 3 and 4. The results of this analysis emphasize the robustness of regression results when dealing with dichotomous dependent variables, and suggest that the concern frequently expressed by readers on this score is seldom justified.

For readers accustomed to standardized regression coefficients, I can report that comparing, for example, average regression coefficients for a Socioeconomic Resource Level (SERL) variable of education and income, as used by Verba et al. (1978), in the equations yields a standardized beta coefficient of .33 between SERL and vote in the U.S., and only .05 between SERL and vote in 10 other nations.

The greater power of education effects in the United States might lead us to suspect that the education variable was being measured inadequately or differently in different countries. While we cannot disprove this possibility, some persuasive evidence that it is not so is offered in the two right-hand columns of Table 3. Here, the model is applied to a dichotomous measure of political discussion. The discussion results are striking for the great similarity between the impact of education in the United States and the other democracies, and are a tribute to the comparability of the Barnes and Kaase (1979) coding of education level. Not only does the probability of discussing politics rise with each increment of education, but the effects are extremely similar across countries.[6] This comparison seems powerful evidence that the voting participation process, but not the general process of personal involvement, is quite different in the United States.

Not only education, but age, shows a different relationship to voter turnout in the United States and the other countries. In the United States, each age increment shows increased probability of voting. In the other nations, the effects are very great going from the first to the second age group, but quite weak thereafter. On the other hand, the effect of party identification on voter turnout, and on discussion, is about the same in the United States as in the European average. Women voted somewhat less often than men in the United States, while the average difference was very small in other countries. Political discussion showed a sex gap in all countries, but the American difference was much less than that in the other democracies.[7]

The comparison of averages in Table 3 does, of course, blur differences among the non-American nations. Moreover, we need clearer evidence on the attitudinal processes involved in getting voters to the polls. In Table 4 we see individual participation models for the eight nations in the 1973–1976 Political Action study plus Canada in 1974, and including measures of interest and efficacy variables used in many American studies. For the sake of simplicity, political trust is not included here; its effects are quite weak, although in the predicted direction in most countries. The top of the table shows, again, the increased amount of voting participation expected from each increment of education, controlling for interest and efficacy as well for sex, age, and party identification. Toward the bottom of the table we see the predictions for interest and efficacy. Each of the nine nations is presented separately, as we are not controlling for system-level effects. (The efficacy variable is a two-question variable, including the "no say" item shown in Table 1, exactly as used in Abramson and Aldrich, 1982.)

The data in Table 4 are complex, but rich in information. The first point to note is that the individual voter participation processes in Austria and, especially, Italy (shown at the far right of the table) are rather different from those in the other countries. The attitudinal variables, particularly interest, but also efficacy, education, and even party identification, have much less effect in these two countries. And age is notable for the very great increase between the 21–24 group and the next older one, with very little subsequent change. It seems likely that voter participation in Austria and Italy is dominated primarily by institutional effects. Both countries have substantial compulsory voting, and Austria has an extremely well-organized and penetrative party system, so these patterns are not too surprising. Moreover, the extremely high

Table 4. Individual-Level Explanations of Voter Turnout: Predicting the Increase in Probability of Voting from Demographic and Attitudinal Variables in Nine Nations

	Predicted Increment in Voter Turnout Relative to Base-line Group (%)[b]								
Independent Variables	USA	BRIT	WGER	NETH	SWITZ	FIN	CAN	AUST	ITALY
Education Level[a]									
Basic	—	—	—	—	—	—	—	—	—
Lower	- 5	- 3	0	- 1	10*	- 3	1	1	0
Extended Lower	2	- 2	2	2	7*	0	5	1	3
Middle	15*	1	4	7	9*	5	9*	- 0	0
Post-Secondary	21*	6	3	7	15*	4	5	2	- 4
Sex									
Male	—	—	—	—	—	—	—	—	—
Female	- 3	1	- 0	8*	- 4	0	- 0	1	0
Age									
20–25	—	—	—	—	—	—	—	—	—
26–29	10*	12*	14*	13*	9*	25*	3	40*	48*
30–39	19*	26*	16*	9*	16*	32*	6	39*	51*
40–49	23*	36*	15*	15*	23*	31*	15*	40*	51*
50–59	29*	34*	16*	19*	25*	35*	10*	40*	52*
60+	37*	33*	15*	21*	20*	35*	15*	40*	52*
Party Identification									
No	—	—	—	—	—	—	—	—	—
Yes	13*	24*	8*	33*	21*	9*	4*	4*	3*
Political Interest									
Not at All	—	—	—	—	—	—	—	—	—
Not Much	17*	9*	5*	8*	19*	9*	24*	2	2
Somewhat	30*	15*	7*	12*	34*	15*	34*	1	4*
Very	32*	18*	8*	8*	33*	14*	36*	2	- 0
Political Efficacy									
Low	—	—	—	—	—	—	—	—	—
Mixed	6*	3	2	2	8*	1	- 1	1	- 3*
High	9*	6*	1	7*	6	- 0	- 0	1	- 4*
Reported Turnout (%)	74	79	94	85	59	90	82	96	95

[a] See Note to Table 3 for education levels.

[b] See Note to Table 3 for source of estimate.

* indicates that turnout of group was significantly above base-line group (.05). N of cases from 1030 to 2149.

reported turnout levels of 95% leave limited room for attitudinal effects to have play.

The second point to note about Table 4 is that the six "middle" countries, with automatic registration but without compulsory voting, manifest voter process models that seem rather similar. Naturally, we do find substantial variation—as we would expect from the measurement and language differences, the rather small subgroups in some categories, and the very high reported turnout levels (over 90% in Germany and Finland). But in each country we see sharp, slightly curvilinear effects of political interest and, less consistently, efficacy. Party identification, although varying in magnitude, is a significant direct predictor of turnout in each country.[8] Age has a generally positive and curvilinear effect, although the timing of drop-off varies. Sex is an insignificant predictor of turnout in all countries except the Netherlands, after interest and party identification are taken into account. Finally, we see that after political interest and efficacy have been included in the model, education has very weak and often insignificant effects on turnout. In most countries a somewhat weak initial effect of education (summarized in Table 3) is further reduced by taking account of political interest. Scattered and collectively significant small effects do remain, particularly at the middle and higher education levels.

Considering the American model in comparison with the other six nations without compulsory voting suggests both commonality and difference. On the one hand, attitudinal effects are rather similar. Party identification has an effect which falls about at the average of the other democracies. Interest is similar in strength, if on the higher side, and similar in its curvilinearity (not much impact by the increase from "somewhat" to "very" interested). Efficacy has an effect in the United States that is somewhat above the democratic average. Sex is insignificant in its effect on turnout after attitudinal variables are taken into account.

On the other hand, the direct effects of age and, especially, education are much greater in the United States than in any of the other countries. The age variable is notable for its continuing impact as we move to increasingly older groups. The impact is about average as citizens age from 20 to 39, but continues in the U.S., while becoming weaker in the higher age categories elsewhere. It seems likely that the lesser mobility of the older age groups, in conjunction with the unique American registration system, plays a major role in accounting for this continuing impact of age in the United States (Squire, Glass and Wolfinger, 1984; Wolfinger and Rosenstone, 1980, ch. 3). Unfortunately, a variable for length of residence is not included in the eight-nation study.

Most distinctive of all is the direct impact of education on American voter turnout. While the effect of education shown in Table 3 is reduced somewhat with interest and efficacy in the model, the direct effects are still quite powerful, with high school graduation worth 13% and college work another 6%. Not only does the United States have the most educated citizenry, but education has much more direct impact on voter turnout.[9] It seems very likely, although we cannot demonstrate it directly, that the difficulty of registration in America is also responsible for this remarkable distinctiveness of American voting processes. The great weakness of the party system in its organization and linkage to social groups may also enhance the value of personal characteristics and resources.

Of course, the overwhelming point to make about the models shown in Tables 3 and 4, when compared to the evidence on the cultural environment from Table 1, is that they all lead us to expect high voter turnout in the United States. The American electorate is highly educated, has above average levels of party identification, and is more interested and more efficacious than citizens in other democracies. The relatively low levels of trust are not as important as these other American advantages.

These individual-level modeling efforts redirect our attention to contextual factors operating largely, it would seem, at the national level. It is possible, of course, that incorporation of attitudinal and demographic variables here unmeasured would succeed in explaining the differences across nations in level of voter turnout. It would be especially reassuring to see a variable for citizen duty available across countries.[10] But the comparisons with political discussion suggest that the present variables, at least, are being measured fairly well, and that they do not account for the relatively low levels of American voter participation.

Effects of Misspecification in the Aggregate- and Individual-Level Models

Thus far we have estimated the importance of aggregate and individual characteristics in predicting voter turnout in separate models. To use the estimates in evaluating the factors depressing or encouraging American voter turnout, we need some assurance that each model is being reasonably well specified, despite absence of the variables in the other model. We cannot fully solve this problem without comparable attitude surveys in all the democracies, but we can gain some confidence by conducting the individual and aggregate analysis jointly in the subset of countries for which we have both kinds of data, and within which the individual models were fairly similar. We can pool the 7000 cases in the six countries that have both automatic registration and voluntary voting, replace the names of countries with their values on aggregate contextual variables, and estimate the coefficients with both individual and contextual variables operating together.

The problem in this approach is that we still have only six countries, despite 7000 cases, and some of the contextual variables interact badly, creating unstable estimates. Nor do we have the degrees of freedom to enter many contextual variables simultaneously. We can, however, get some sense of the specification problem by using only a few of the aggregate variables. We can first run the model with these variables only, then add the attitudinal variables and see if the estimates change greatly. The process is reversed to ascertain the stability of the attitudinal estimates. The results of these procedures are relatively reassuring.

Consider first the aggregate coefficients. I ran the model with dummy variables for Switzerland, the age categories, and district competitiveness (coded single-member district, multidistrict proportional representation [PR], national PR). Then I added the full set of individual-level variables for education, party identification, interest, and efficacy. The estimates for district competitiveness increased slightly after adding the individual-level variables. (The resulting model is that shown in Appendix 2.) Repeating this analysis using party-group linkages rather than district competitiveness also led to an increase in the party-group linkages coefficient by about 20% after including the attitudinal variables. Although the size of the coefficients is, naturally, rather different than the results in Table 2, because of the small number of cases and the inclusion of only one variable at a time, the analysis suggests that we can use the estimates from Table 2 without being too worried that they are badly biased by the absence of the attitudinal variables. Naturally, we would prefer to use the estimates based on the full set of democracies, rather than the six in the survey.

The results for the individual variables are also fairly robust, but show some need for caution. If we pool the six countries and run the individual-level variables alone, we get estimates for party identification, interest, and efficacy that are fairly similar to results including the aggregate variables (either as in Appendix 2, or with dummy variables for five of the countries). But the education effects, always somewhat weak and inconsistent in these countries, are estimated to be weakly negative after interest and efficacy are taken into account. Not until we include a variable for district competitiveness, or the country dummies, do we get the weakly positive education estimates we might expect from averaging the coefficients in Table 4.

The results of this exploration of the

specification problem suggest that we can use the estimates directly from Table 2 for the contextual variables. But estimates for the individual-level variables must either explicitly include contextual variables, as in Appendix 2, or use country dummies or an averaging procedure that implicitly takes account of such effects.

Estimating the Effects of Cultural and Institutional Setting on Relative American Voter Turnout Levels

The coefficients estimated in the previous section can be used to analyze the effect of individual and institutional variables on levels of voter turnout in the United States as compared with other democracies. We must, however, keep in mind the unique nature of the American model, as shown in Tables 3 and 4. Because of the unique difficulty of registration in the United States, if for no other reason, it is difficult to be sure how the contextual variables affect American participation under present conditions. We shall approach the problem by using the coefficients estimated in the previous section to predict how turnout in the average modern democracy would change if its attitudinal and competitive conditions were similar to those in the United States.

Beginning with the individual-level variables, we have estimated the increment to turnout created by increased levels of party identification, education, interest, and efficacy. By comparing the percentage of the American citizenry with those characteristics with the percentage in the average democracy, we can multiply the difference by the estimated turnout increment to see the predicted effect of the average democracy developing a political culture similar to the American. For example, 84% of the American respondents stated a party identification, compared to 78% of those in the other democracies. If we multiply the 6% American advantage in party identification by the predicted increment of 14% (from Appendix 2) for individuals in the average democracy having a party identification, we estimate that increasing party identification in other countries to the American level would increase their average voter turnout by .85%—a bit under a 1% gain in turnout. In the case of education, interest, and efficacy, we do the analysis for each category above the base-line category, and sum the results to see the impact of the full distribution on the expected levels of voter turnout. The analysis is shown in the top half of Table 5.

While the analysis is somewhat complex, the results are simply summarized. If citizens in the average democracy were as interested in politics as Americans, voter turnout would increase by 2.2%; American levels of efficacy would increase turnout by .5%. The education coefficients estimated in Appendix 2 would increase turnout by 1.6% if the average democracy reached American education levels. These education estimates, as noted, are somewhat unstable; using average increments from Table 4 leads us to predict a 2.7% increase; using a model with country dummies leads to a figure between these. We should note that while the education coefficients are small, the huge American advantage in education levels has a notable effect. Over all, the United States is advantaged by its political culture. If the average democracy had a political culture as facilitating to voter turnout as American education and attitudes, we would expect turnout to increase by about 5%.

We can do the same thing with the institutional factors, using the models in Table 2. The presentation and analysis are slightly simpler here, because we use the linear estimates from Table 2, and can make predictions on average differences between the U.S. and the average democracy. If the other democracies had

Table 5. Predicting Changes in Voter Turnout if the Average Democracy Had Automatic Registration, *but Individual and Institutional Characteristics Similar to the United States*

Independent Variables	Distributions on Independent Variables[a] (%)		United States Advantage in 1970s (%)	Estimated Regression Coefficient[b]	Predicted Change in Turnout in Average Democracy (%)
	Other Nations	United States			
Education					
Basic	36	5	—	—	—
Lower	22	16	- 6	- .011	.07
Extended Lower	21	13	- 8	- .003	.02
Middle	11	31	20	.026	.52
Higher	10	35	25	.039	.98
Party Identification					
No	22	16	—	—	—
Yes	78	84	6	.142	.85
Political Interest					
None	17	9	—	—	—
Not Much	27	21	- 6	.118	- .71
Some	38	44	6	.189	1.13
Very Much	17	26	9	.197	1.77
Political Efficacy					
No	49	34	—	—	—
Some	29	29	0	.024	0
Yes	22	37	15	.033	.50
Age					
Over 34 Years	63	58	- 5	.380	- 1.90
Nationally Competitive Election Districts Scale	2.76	1	- 1.76	1.800	- 3.20
Party-Group Linkage Index	36	13	-23	.450	-10.40
Net Predicted Changes due to:					
Individual Level Variables of Education, Party, Interest, Efficacy					5.1
Aggregate Age Distribution					- 1.9
Institutional Variables					-13.6

Sources and Variables: See Tables 1, 2, 3, and Appendices 1 and 2.

[a] Institutional variables from Appendix 1, excluding compulsory voting nations. Attitudinal variables for the countries in Appendix 2.

[b] Coefficients from Table 2 and Appendix 2.

the American levels on competition-encouraging constituencies and party-group linkages, their turnout would be predicted to decrease by about 13%. The weak American linkages between parties and groups (and the associated weak party organizations) would reduce turnout by 10%. The low competitiveness of some American electoral constituencies would reduce turnout by about 3%. (The variable for changes in the chief executive, insignificant in both decades as shown in Table 2, is not included.) The age level increased turnout in the 1960s by a small amount, but decreases it about 2% in the 1970s, with the American lowering of voting age and the age bulge among the young. The net effect of all the variables in Table 5 is to lower turnout by about 10%, the American attitudinal advantage being outweighed by the institutional disadvantage on a 13 to 5 basis, with age adding another 2% disadvantage.

If we make the more heroic assumption that adopting automatic registration would create an American voting process like those in the other democracies, we would predict, then, that such registration would lead to American turnout levels some 10% below those of the cross-national averages. Recalling that turnout in the countries without compulsory voting (other than Switzerland) averaged 80%, we see that such changes would mean American turnout of the age-eligible would be increased from 54% to 70%. As discussed in Appendix 3, the presence of resident aliens, ineligible to vote, would limit this by at least 2%, to about 68%. Given the American institutional disadvantages (apart from registration), the U.S. would still have one of the lowest turnout levels of any democracy, tied with Canada and ahead of Switzerland, but the gap would be far less.

In comparison to present American voter turnout levels, the analysis implies that if the United States adopted automatic registration, or something similar, turnout might be increased by 14%.[11] This estimate is not inconsistent with that based on cross-state comparisons by Rosenstone and Wolfinger (1978), predicting that voter turnout in the early 1970s would have increased by about 9% if all states had had registration laws as facilitating as those in the most permissive states. It also fits reasonably well with the point made by Glass et al. (1984) that average turnout in states with election-day registration in the 1980 election was about 66%, some 13% above the national average. None of these states, of course, had automatic or compulsory registration. (The 1980 comparison, however, does not consider other attitudinal and institutional characteristics of the states with election-day registration.)

As Glass et al. (1984) and others have argued, getting most American citizens registered would lead to a major increase in American voter turnout. However, the present analysis suggests that it would not lead the U.S. to overtake most other democracies in voter turnout. Assuming that the U.S. does not wish to introduce compulsory voting, the other institutional factors are probably hard to implement. To make the presidential elections competitive across the country by doing away with the electoral college would probably help somewhat, but not as immediately as the 3% constituency factor in the model indicates, as it is surely capturing some party system effects. And the single-member district effects at legislative and lower levels would remain.

A final point here concerns changes over the last two decades. As has been noted by various scholars, the attitudinal characteristics that enhance participation in the U.S. have declined sharply since the early 1960s. If the 1960–64 levels of American education, partisanship, interest, efficacy, and trust are compared to the European averages of the 1970s (shown in Table 1) the U.S. ranks first in each measure by far. As we saw in Table 4, these individual characteristics are particularly important for voting in the American context. The more negative U.S. dummy coefficient in the 1970s in Table 2 seems to reflect the degree to which the attitudinal advantages compensated for the difficult U.S. registration conditions in the 1960s, but not in the 1970s.

Concluding Comments

In closing it is perhaps useful to separate the firm conclusions of this analysis from the more speculative ones. Comparison of voter turnout in the United States with voter turnout in other industrialized democracies leads to four observations in which we can be quite confident:

(1) Measured as a percentage of the population of voting age, voter turnout in the United States is very low in comparison with the other democracies. It was well below average in the 1960s and has declined even further, while average turnout in the other democracies has been stable at about 80%.
(2) The American attitudinal environment is, nonetheless, rather favorable to citizen participation of all kinds, including voting, although less so than in the early 1960s.
(3) The American legal and institutional environment is inhibiting to voter participation.
(4) As a form of participation, voting is particularly influenced by institutional factors, although attitudes are relevant.

Some other forms of participation, such as political discussion, are much less influenced by the institutional setting, and Americans are comparatively highly active in these forms of political involvement.

Although we cannot be quite as confident, it seems very likely that the unique American registration laws, which require frequent citizen initiatives from a mobile population, play a substantial role in depressing American voter turnout. It is also likely that these laws are responsible for the unusual degree to which education and other socioeconomic resources are directly related to voter turnout in the United States, even beyond their role (found virtually everywhere) in creating the attitudes of interest and efficacy which encourage participation.

The specific weights that have been attributed to the various factors are much more speculative, for a variety of technical reasons. This analysis suggests that in comparative perspective, the United States would be advantaged about 3% by its configuration of attitudes (especially political interest) and another 2% by its education level, but disadvantaged 2% by the age levels, 13% by the other institutional and party system factors, and up to 14% by the registration laws. There has been a marked decline in the attitudinal advantage over the last two decades. Although the amount of its effect is somewhat in doubt, the registration laws are probably the most easily altered factor, as well as perhaps the most important. Changing these laws would still leave the United States with below-average voter turnout, but the gap between the United States and the cross-national average would be greatly reduced. Changing the structure of party competition to mobilize lower class voters, for example, is probably much more difficult, although blacks represent an obvious target of opportunity for the Democratic party.

A full-scale analysis of the consequences of voter turnout is beyond the scope of this paper. We can, however, briefly conclude with a comment on two aspects of the problem: system legitimacy and voter quality. A substantial debate exists in the democratic theory literature concerning the implications of high levels of voter turnout for the legitimacy and stability of democratic systems. On one hand, theorists favoring citizen participation have argued that higher levels of turnout reflect and encourage political legitimacy and citizen support. On the other hand, theorists concerned about democratic stability have pointed to the often undemocratic values of the less educated, and the high levels of turnout in such unstable systems as Weimar Germany and postwar Italy, to argue the dangers of high voter turnout levels.

Recent empirical analysis suggests that the theorists favoring participation have the best of the argument. Analysis of voter turnout in 29 democracies clearly shows a strong association between higher turnout and less citizen turmoil and violence. After controlling for various economic, constitutional, and party system variables, higher levels of turnout still seemed associated with less frequent citizen riots and protests, although not related to deaths by political violence or the overthrow of democracy (Powell, 1982, ch. 10). On the other hand, the countries with higher levels of turnout, at least among the more industrialized countries, did tend to have less durable tenure of the chief executive. From the American perspective, where a presidential system usually creates substantial executive durability in any case, the balance of the evidence would seem to favor higher levels of citizen voter involvement. There should be some channeling of citizen discontent out of the streets and through legitimate political channels without loss of executive stability.

The argument about the quality of the voter can be approached from two directions. On one hand, because of the effects of social and economic resources under the present system, there is little doubt that adopting automatic registration or other measures to encourage turnout of the less well-off would bring to the polls a total electorate somewhat less interested, efficacious, and informed than the present voters. We do not want to overstate this fact, because we have already estimated that such changes would only increase turnout to less than 70%, and some small effects of interest and education in encouraging voters remain. But some decrease in the present levels of voter sophistication (not exactly overwhelming in ideal terms) would probably occur. On the other hand, we must recall that by comparative standards, the American electorate is extremely interested and involved. Even the total mobilization of all American citizens would mean a voting group that is more interested, efficacious, and more likely to engage in political discussion and other activity than are present voters in most other democracies.

Appendix 1. Voter Turnout and Institutional Characteristics of Twenty Democracies in the 1970s

Country	Average Turnout as Percent Eligible	Average Registered as Percent Eligible	Compulsory Voting	Eligible Required to Register	Frequency of Change in Chief Executive[a]	Type of Electoral District[b]	Average Strength of Party-Group Linkages
Australia	86	91	Yes	Yes	3	1	33
Austria	88	96	No	Automatic	3	3	42
Belgium	88	95	Yes	Automatic	2	3	51
Canada	68	93	No	Automatic	4	1	29
Denmark	85	98	No	Automatic	4	4	43
Finland[c]	82	100	No	Automatic	2	3	48
France (presidential election)	78	91	No	No	3	4	33
West Germany[d]	85	94	No	Automatic	2	4	34
Ireland	77	100	No	Automatic	4	2	25
Israel	80	100	No	Automatic	2	4	—
Italy	94	100	Yes	Automatic	2	3	40
Japan	72	100	No	Automatic	1	2	30
Netherlands	82	98	No	Automatic	2	4	45
New Zealand	83	95	No	Yes	3	1	40
Norway	82	100	No	Automatic	3	3	37
Sweden	88	97	No	Automatic	3	4	42
Switzerland	44	85	No	Automatic	1	3	43
United Kingdom	75	100	No	Automatic	4	1	33
United States (presidential election)	54	61[e]	No	No	4	1	13
Spain	78	100	No	Automatic	—	3	42

Sources: For registered and voted, Mackie and Rose (1982) and *European Journal of Political Research.* Age-eligible population calculated from United Nations Demographic Yearbooks, 1979, 1981, 1983 (based on census data, interpolated to election year).

[a] 4 = Chief executive changed party hands 50% of elections 1960–80; 3 = clear changes, but under 50%; 2 = partial changes only; 1 = no changes in parties controlling chief executive.

[b] 4 = national election PR or national pool for some legislative seats, or simple national presidency vote; 3 = large district PR; 2 = PR with three to five members per district; 1 = single member or winner-take-all districts.

[c] Excludes Finns living outside of Finland from voted, registered and eligible.

[d] Excludes West Berlin (not eligible to vote in West German elections).

[e] Estimated from University of Michigan Survey Research Center study; see Katosh and Traugott, 1981.

Appendix 2. A Comparison of Three Models of Multivariate Analysis of Voter Turnout in Six Nations:[a] Regression, LOGIT, PROBIT

Independent Variables	Slope Coefficients			Predicted Turnout (%)			Simple Turnout (%)
	Regression	LOGIT	PROBIT	Regression	LOGIT	PROBIT	
Intercept	.347	4.15	4.08	84	84	84	84
Education[b]							
Basic	—	—	—	83	83	83	85
Lower	-.011	-.009	-.016	82	83	83	82
Extended Lower	-.003	.021	.014	83	83	83	82
Middle	.026	.137	.139	86	86	86	83
Post Secondary	.039	.206	.208	87	87	87	88
Age							
20–25	—	—	—	66	67	66	65
26–29	.113	.342	.402	77	77	77	77
30–39	.173	.581	.662	83	83	83	84
40–49	.221	.787	.887	88	88	88	88
50–59	.218	.783	.890	88	87	88	87
60+	.217	.785	.895	87	88	88	87
Party ID							
No	—	—	—	73	74	74	68
Yes	.142	.516	.583	87	87	87	88
Interest							
Not at All	—	—	—	70	72	72	66
Not Much	.118	.353	.404	82	82	82	79
Somewhat	.189	.671	.747	89	89	89	90
Very	.197	.805	.863	90	91	90	94
Efficacy							
Low	—	—	—	82	82	82	81
Mixed	.024	.107	.123	85	85	85	86
High	.033	.148	.174	86	86	86	88
Electoral							
SMD	—	—	—	75	74	74	81
Multi-District PR	.112	.522	.575	87	86	86	75
National PR	.125	.628	.685	88	88	88	92
Switzerland							
No	—	—	—	87	87	87	87
Yes	-.250	-.882	-.997	62	62	62	62

[a] Pooled data from Britain, West Germany, Netherlands, Switzerland, Finland and Canada. N = 7191. Excludes cases missing on any variable. LOGIT and PROBIT from MVS/OS version of SPSSX; the program adds 5 to the PROBIT and divides by 2 and adds 5 to LOGIT; account is taken of this in the predictions.

[b] See the note to Table 3.

Appendix 3

The problem of fair comparison of voting turnout figures raises many theoretical and technical issues. One of these is worth special comment here: alien residents in the population. In most democracies voting eligibility is limited to citizens. Population figures, on the other hand, usually include noncitizens resident in the country for a year or more. Countries vary substantially in the percentage of such aliens, largely depending on the presence of jobs and ease of access to draw alien workers and their families, but also depending on their naturalization policies in denying or facilitating attainment of citizenship by resident aliens. If we accept the legitimacy of the denial of voting eligibility to aliens, then the countries with substantial alien populations are penalized in column 2 of Appendix 1. Unfortunately, we do not have good data on percentage of residents counted by census who are aliens of voting age, and cannot systematically adjust our turnout data to remove them. For the United States we do have census estimates based on self-reported citizenship. This percentage probably understates noncitizenship; we do not know by how much. The census study figures (U.S. Bureau of Census, 1978, 1982) indicate 3–4% of the population of voting age in the United States were noncitizens in the 1970s. Adjusting the U.S. turnout figures to exclude these noncitizens would increase turnout of age-eligible Americans by 2% —closing only slightly the gap between American turnout and the democratic average. As we can see in Appendix 1, this adjustment would not alter the position of the U.S. relative to most other democracies.

Moreover, while it is clear that some countries in Appendix 1 have fewer aliens than the United States, figures for West Germany, France, Belgium, and Switzerland indicate that they had more aliens in the resident population than did the U.S. (Britain also had more, but special provisions affecting immigrants from the Commonwealth enfranchised many of them rapidly.) The best figures and most aliens were for Switzerland, where the foreign worker population increased from 10% in 1960 to 16% in 1970. Moreover, a 1975 study indicated that 80% of these Swiss immigrants had lived in Switzerland for over seven years, although very few were able to become citizens under the difficult Swiss rules, which include a twelve-year residency requirement (Miller, 1981; Castles and Kosack, 1973; Krane, 1979). Excluding aliens from both Switzerland and the U.S. brings Swiss turnout nearly to the American level, and accounts for virtually all of the Swiss unregistered population of voting age. Thus, the resident alien problem does not seem to be a serious problem in American comparisons with other democracies, although Switzerland is rather sharply affected. (The slight decline in turnout as percent of resident age population in West Germany in the 1970s may well also be caused by foreign workers.)

Notes

The research for this paper was carried out while the author was supported by a John Simon Guggenheim Memorial Foundation Fellowship. An early version of this paper was presented at the Conference "Where Have All the Voters Gone?" at the University of Chicago, April 26-28, 1984. I wish to thank the following for their assistance: The Guggenheim Foundation and the University of Rochester for their support; the ICPSR at the University of Michigan for providing the survey data; Samuel H. Barnes, Max Kaase, and their associates as well as the Zentralarchiv f. empirische Sozialforschung at the University of Cologne for making available the political action surveys; Jacques-Rene Rabier and

Ronald Inglehart for making available the Euro-Barometer surveys. Lynda W. Powell and Steven J. Rosenstone provided particularly useful comments and advice. None of the above bear any responsibility for the analyses or interpretation presented here.

1. For average gammas between attitude variables and turnout in the United States between 1960 and 1976, see Miller, Miller, and Schneider (1980). The correlation between trust and turnout is .12, compared to .37 for efficacy and .50 for interest.

2. See Niemi, Stanley and Evans (1984), and the references therein. For sake of simplicity, I am here ignoring the slight turnout decline among the oldest age groups.

3. See Crewe (1981) and Powell (1980) for alternative measures. See Powell (1980) for most sources used to estimate party-group linkages. These data were extended with data from the political action surveys and Euro-barometer (Rabier and Inglehart, 1981) study cited above, and with calculations from tables appearing in Flanagan (1984), Holmberg (1981), Levine and Robinson (1976), Linz (1980), and Valen (1979).

4. The one demographic group in the United States with highly differential party support is black Americans. I have not included black support in the American party-group linkage calculations for two reasons. First, because groups as small as 10% of the population are not included in the analysis for other countries, and would increase the linkage numbers in most countries if they were. Secondly, although Verba et al. (1978, ch. 10) and others have shown that blacks do overparticipate as a social segment, relative to expectations from their socioeconomic resources, they seem relatively unmobilized considering their potential value to the Democratic Party. While this may be changing in 1984, it suggests that party-group linkages depend on party mobilization efforts to take advantage of differential group linkages, and are not purely individual-level phenomena.

5. For a more extensive discussion of the Swiss case see Powell (1980) and Appendix 3.

6. See the very comparable relationships between socioeconomic resources and "political involvement" reported by Verba et al. (1978, p. 75) in their seven rather different nations, and the marked contrast they, too, found with voting participation.

7. See also Almond and Verba (1963) and Verba et al. (1978, ch. 12) for similar findings on differences between males and females.

8. Measurement of partisanship is a matter of current controversy too complex to review here. In fact, it seems surprising that the results for partisanship and turnout are as consistent as they are, given the different meanings and stability associated with partisanship across countries. But the coefficients in Table 4 do show some notable differences in the impact of party identification on turnout. Measurement problems may well be responsible. The present analysis has been replicated using a measure of partisan intensity rather than a simple dichotomous partisan identification variable, with very similar results. In the case of Canada, it should be noted that the general political interest question was not available in the Canadian study. A question about interest in the campaign was used instead, which had a particularly weakening effect on the original Canadian party identification coefficient. However, that coefficient was well below average even without the other attitudinal variables. Efforts to use a provincial or combined party identification measure for Canada, rather than the federal party identification used here, did not change the results.

9. For similar results in a different study, using different surveys, see Verba, Nie, and Kim (1971, pp. 75-79).

10. American voting research suggests that citizen duty is the best predictor of voter turnout, and has not changed greatly in magnitude or power over the past 20 years (Miller et al., 1980). I have not been able to find comparative studies using citizen-duty variables in other countries. The Almond-Verba study (1963) does examine responses to a question on obligations that citizens owe their countries. Americans and Germans report rather similar frequencies of mentioning voting as an obligation—levels higher than the British, Italian, and Mexican respondents. It seems unlikely that duty is a more powerful mobilizer in Europe than in the United States.

11. It must be emphasized repeatedly, of course, that other, unmeasured, attitudinal and institutional variables may account for part of the difference. We have, for example, no measure of citizen mobility. Although its effects should be less under conditions of automatic registration, we would still expect less turnout from the highly mobile. This and other unknown factors are included in what here is ascribed to registration laws. For that reason the estimate may well be on the high side.

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PRESIDENTIAL COATTAILS AND MIDTERM LOSSES IN STATE LEGISLATIVE ELECTIONS

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The president's party consistently loses partisan control of state legislatures in midterm elections, a pattern similar to the loss of seats in the U.S. House of Representatives in midterms. This study examines presidential coattails as a possible explanation of these losses. Aggregate state legislative election outcomes between 1944 and 1984 in 41 states are examined. The analysis indicates that the president's party gains seats in presidential elections in proportion to the presidential vote in a state, and subsequently loses seats in midterm elections also in proportion to the prior presidential vote in the state. The presidential coattail and the midterm repercussion effects are evident even when gubernatorial coattail effects are introduced, but are fairly modest in states lacking competitive parties.

Despite their number and importance, state legislative elections have not received a great deal of scholarly attention. One recent exception to this neglect is John Bibby's study of state legislative midterms (1983a, 1983b). Bibby found a definite pattern in midterm changes in partisan control of state legislatures. The president's party regularly loses control of state legislatures at the midterm, much as it loses seats in the U.S. House of Representatives. In each of the 9 midterms from 1950 to 1982, the presidential party suffered a net loss in the control of state legislative chambers. These losses ranged from 8 chambers in 1970 to 24 chambers in 1974.

This analysis proposes to extend and explain Bibby's findings. First, this study will extend Bibby's analysis of changes in control of the state legislature by examining changes in the percentage of seats held by the president's party. Bibby's findings regarding changes in partisan control are quite probably just one manifestation of a more general loss of seats for the president's party. Second, the study's main purpose is to determine if presidential coattails explain midterm losses. Other theories of presidential midterm losses in Congress have been suggested, notably Tufte's referendum on economic conditions and presidential popularity thesis (1975, 1978). However, the coattails theory (Bean, 1948; 1950; 1972, pp. 50–60) and, in its more sophisticated form, Campbell's "surge and decline" theory (1966) have been shown to go a long way toward explaining the pattern of midterm loss in Congress (Campbell, 1985). The coattails thesis is that in a presidential election year a successful presidential candidate assists in the election of his party's slate of candidates, whether they be candidates for the U.S. House of Representatives or for state legislative seats. At the midterm many of these candidates, boosted into office in the prior election with help from the top of the ticket, lose when they run for reelection without the benefit of presidential coattails.

One might argue that presidential coattail effects in state legislative contests don't make sense. State legislators and presidents deal at different levels of government and for the most part are concerned with different issues. Nevertheless, parties are generally known by the presidential candidates they nominate, and candidates for state legislative seats are a good deal less well known to voters than the congressional candidates who ride presidential coattails. Therefore, even though one can make a case that there may not be good policy reasons for coattails to affect state legislative races, many voters may use their presidential vote as a guide in casting a vote for the state legislature because they lack other information (Hinckley, Hofstetter, and Kessel, 1974).

The analysis is divided into four major sections. The first section examines state legislative seat changes in presidential election years. The central question in this portion of the analysis is, to what extent do presidential coattails extend to state legislative elections? The second section examines state legislative seat changes in midterm elections. Again, the central question concerns the possible repercussions of presidential politics for state legislative elections: To what extent do seat changes at the midterm reflect the removal of presidential coattails and the return to normal partisan politics?

Within each of these two sections the analysis will proceed in two steps. First, the extent of seat gains or losses for the president's party will be examined for all states in both presidential and midterm years. The distribution of gains and losses should provide an initial indication of whether coattails exist at the state legislative level, and whether Bibby's findings are a manifestation of a pattern of state legislative seat loss for the president's party at the midterm. Second, models will be constructed to account for seat changes in both presidential and midterm elections. At the state level the models will be examined with aggregate state election results over a period of elections. The state-by-state analysis is designed to control for the numerous idiosyncracies of the states that might otherwise mask the more general effects.

The third section of the analysis involves an examination of variations in coattail and midterm effects from state to state. Is there systematic variation, and what are its sources? In particular, do differing levels of party competition account for differences in presidential coattail effects and their midterm repercussions. The fourth section compares presidential coattails to gubernatorial coattails in state legislative elections.

The Data

Data are drawn at the state level from the 11 presidential elections and the 10 midterm elections from 1944 to 1984. Two states, Nebraska and Minnesota, are excluded because their state legislatures were nonpartisan for at least half of the time period under study. An additional 7 states are set aside because of peculiarities in election scheduling. Four of these states —Alabama, Louisiana, Maryland, and Mississippi—have four-year terms rather than the usual two-year terms for state legislators (Council of State Governments, 1968, p. 49). Mississippi and 3 other states—Kentucky, New Jersey, and Virginia—hold their state legislative elections in odd-numbered years rather than the standard even-numbered years (Jewell and Olson, 1982, p. 16). Two states, Alaska and Hawaii, have been included although data on only 6 midterm and 7 presidential elections are available. The active data set covers a total of 443 presidential elections and 402 midterm elections in 41 states.

Since the analysis is concerned with on-year as well as off-year elections, there are two principal dependent variables. The first is coattail seat gain.[1] This is calculated as the percentage point gain in the seats held by a party before and after the presidential election. For example, if the Democratic party held 30% of a state legislature's seats before a presidential election, and won 40% of those seats in the election, the seat gain for that party would be 10 percentage points. The second dependent variable is midterm seat loss. The midterm seat loss variable measures the change in seats held by the president's party in the midterm election. Like the coattail variable, the midterm seat loss variable is calculated in terms of percentage point differences. The partisan compositions of the state legislatures were extracted from various volumes of the Council of State Government's *The Book of the States* (Council of State Governments, 1942–1982) and from *Congressional Quarterly Weekly Reports* (Donnelley, 1984).

A number of independent variables have also of course been collected. The principal independent variable is the percentage of the two-party statewide vote won by each of the two major presidential candidates in the presidential election. In addition, election results for gubernatorial races have also been collected in both presidential and midterm years in states holding these elections. Election returns were obtained from *Presidential Elections Since 1789* (Congressional Quarterly, 1975) and various issues of the *Congressional Quarterly Weekly Report* (Congressional Quarterly, 1981, 1984; Cook, 1976).

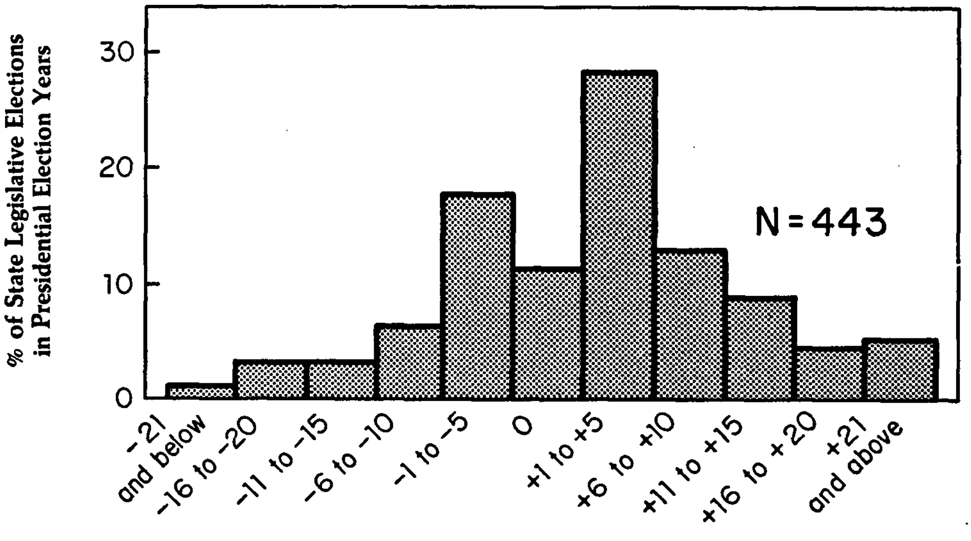
Seat Change in Presidential Elections

As expected, the party winning the presidency also tends to do well in state legislative contests held during presidential election years. The distribution of seat changes in state legislatures after presidential elections is skewed in favor of the party of the winning presidential candidate, much as one would expect if coattails were at work. Figure 1 is a histogram of the frequency of gains and losses for the president's party in presidential elections.

The party winning the presidency is nearly twice as likely to win seats in state legislatures as it is to lose seats. The winning presidential party gained at least 1% of the state legislative seats in 58% of the state elections examined (257 of 443). It lost 1% or more of the seats in 31% of the elections. There was no change in the remaining 11%. The mean seat change in all elections examined was a 3.2% gain for the winning presidential party. While this is a fairly definite pattern consistent with presidential coattails, an even stronger pattern may emerge in a multivariate model addressing less aggregated data. Presidential party losses may well be concentrated in states the candidate winning nationally failed to carry.

A single-equation model was constructed to examine state legislative seat changes in presidential elections more thoroughly. The dependent variable is simply the percentage point change in the seats held by the Democratic party after the presidential election. There are two major independent variables. The first is the percentage of the two-party presiden-

Figure 1. Net Change in the Percentage of State Legislative Seats for the President's Party Following Presidential Elections



Percentage Point Seat Change for President's Party

tial vote won by the Democratic presidential candidates. This is the coattail variable. The presumption is that the stronger the presidential candidate runs in a state, the more help he is to the bottom of his party's slate. Although other measures of the coattail variable have been suggested (Calvert and Ferejohn, 1983; Miller, 1955), the simple presidential vote variable has served quite adequately in previous studies exploring its effect in congressional elections (Campbell, 1985, 1986). The second major independent variable is the Democratic party's strength in the state legislature going into the election. If a party already holds a large percentage of the seats, it should be more difficult to add to that large base. It is a simple arithmetic fact of life that you cannot gain what you already have. Parties having a large initial base are also confronted with the likelihood of diminishing returns. It is likely to be more difficult to win an additional seat if you already hold 99% of the seats than it is if you only hold 30%. The dependent variable and both of the major independent variables are oriented in terms of the Democratic and Republican parties, rather than in terms of the winning and losing parties, in order to capture the full variance of the variables. Orienting the analysis according to the winning or losing parties would artificially restrict variance and attenuate measures of association.

A third independent variable is also introduced into the analysis as a trend variable. Since the data set spans four decades, a great many changes in state and local politics may have occurred that could affect seat changes. For instance, a state's partisan balance may shift over time. It would be very difficult to obtain individual measures of these changes and, given the small number of cases for each state, would be even more difficult to incorporate such variables in the model.

As such, the analysis uses a simple counter variable to take trends into account. The variable is simply the last two digits of the presidential year. Since this variable is meant to control as much as possible for trends that might exist in any given state, and since it is not of any theoretical interest in and of itself, it is only included when it has a statistically significant effect.

Table 1 presents the regression results for each of the 41 states examined. On the whole the model appears to be fairly strong. The model accounts for 63% of the variance in the median state and for more than half the variance in 29 of the 41 states. Moreover, the two main independent variables have their expected effects.

The regressions present pretty convincing evidence that presidential coattails reach down to carry state legislative candidates into office. The coefficient of the presidential vote is positive in 39 of the 41 regressions. This pattern is, of course, extremely unlikely to occur by chance alone.[2] Moreover, even though the analysis could not take into account very many idiosyncracies associated with particular elections in particular states—idiosyncracies likely to blur the true impact of presidential coattails—the presidential vote coefficient is statistically significant ($p < .05$) in more than half of the regressions (22 of 41).

The actual strength of presidential coattails varied a good bit. While small negative coefficients were found in the examination of 2 states, positive coefficients exceeding 1.0 were found in 7 states. The median state had a coefficient of .49. That is, about every 2 additional percentage points of the two-party presidential vote won by a party meant a net gain of 1% of the state legislative seats for that party. Moreover, the presidential vote in states commonly varies by much more than 2 percentage points. The standard deviation of the Democratic presidential vote of the median state is 9.2 percentage points. Thus, in a typical state, a change of one standard deviation in the presidential vote can be expected to precipitate about a 4.5% change in the partisan composition of the state legislature.

Some perspective on the magnitude of these effects can be gained by comparing them to coattail effects in congressional elections. A previous study (Campbell, 1986) found, in elections from 1944 to 1980, that a party could expect a net gain of about 3.2 seats in the House of Representatives for every percentage point gained by the party's presidential candidate. In percentage terms, this translates into a coefficient of .74, only slightly more than the median coattail effect in state legislative contests. Thus, by present estimates, presidential coattails at the state legislative level are only slightly shorter than at the congressional level.

The second and less interesting independent variable, the initial base of the party in the state legislature, also had the expected effect. Negative coefficients were associated with the base variable in 39 of the 41 regressions. The negative base effects were statistically significant ($p < .05$) in 23 of the 41 tests, despite the small number of cases. The median state had a base coefficient of −.58. In less precise terms, holding a large proportion of the seats in a state legislature made gaining additional seats substantially more difficult.

Seat Change in Midterm Elections

According to Bibby's analysis and the coattail model, the pattern of seat changes in midterm elections should be the mirror image of seat changes in presidential years. The president's party should lose state legislative seats at the midterm, as the president's coattails are no longer available to state legislative candidates of the president's party.

Table 1. The Coattail Model of Change in State Legislative Seats in Presidential Elections, 1944–1984

State	Intercept	Presidential Vote	Initial Base	Trend	R^2
Alaska	- .63	.98	- .90	.0105	.98
		(11.29)	(48.73)	(5.26)	
Arizona	- .13	.61	- .29	—	.31
		(2.66)	(3.65)		
Arkansas	.08	- .07	.15	- .0009	.40
		(2.63)	(0)	(3.36)	
California	.04	.01	- .09	—	.03
		(0)	(.29)		
Colorado	- .14	1.05	- .64	—	.70
		(7.82)	(8.48)		
Connecticut	- .21	.55	- .29	—	.53
		(3.06)	(3.87)		
Delaware	- .24	1.11	- .57	—	.64
		(6.09)	(10.60)		
Florida	- .05	.23	- .08	—	.31
		(3.19)	(.56)		
Georgia	.15	.16	- .27	—	.46
		(6.83)	(2.16)		
Hawaii	.03	.48	-1.32	.0098	.94
		(16.93)	(40.72)	(19.76)	
Idaho	- .10	.66	- .46	—	.52
		(8.42)	(3.36)		
Illinois	- .12	.96	- .98	.0026	.81
		(20.40)	(8.93)	(3.62)	
Indiana	-1.22	2.75	-1.23	.0083	.95
		(47.11)	(77.80)	(13.90)	
Iowa	-1.46	2.23	-1.20	.0142	.91
		(53.75)	(22.21)	(15.33)	
Kansas	- .11	.36	- .09	—	.21
		(1.73)	(.28)		
Maine	- .67	.35	-1.19	.0151	.70
		(3.17)	(9.47)	(7.79)	
Massachusetts	- .25	.24	- .13	—	.53
		(5.57)	(3.58)		
Michigan	- .39	.83	- .92	.0072	.89
		(16.31)	(34.79)	(12.25)	
Missouri	- .30	.89	- .91	.0067	.89
		(16.67)	(25.18)	(12.22)	
Montana	- .30	.40	- .97	—	.63
		(.90)	(9.41)		
Nevada	.20	.49	- .62	—	.55
		(1.92)	(8.20)		
New Hampshire	- .19	.46	- .03	—	.69
		(15.94)	(0)		

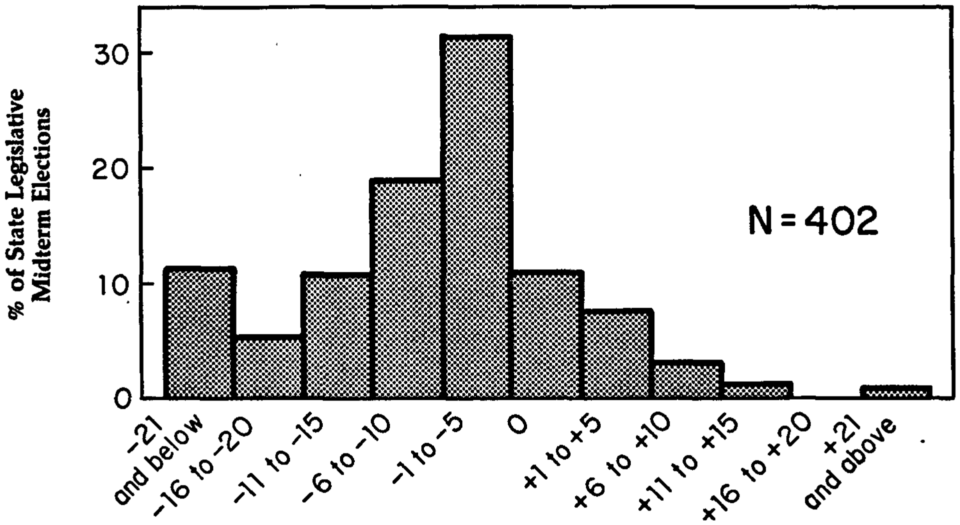
Table 1 (continued)

State	Intercept	Presidential Vote	Initial Base	Trend	R^2
New Mexico	.30	.26 (.36)	.68 (6.71)	—	.46
New York	- .22	.54 (15.28)	-1.12 (24.25)	.0077 (16.30)	.85
North Carolina	.30	.49 (8.03)	- .66 (3.35)	—	.50
North Dakota	- .95	1.34 (14.29)	- .77 (9.53)	.0095 (6.46)	.70
Ohio	- .05	.29 (.47)	- .20 (1.34)	—	.24
Oklahoma	.06	.19 (6.70)	- .19 (1.29)	—	.47
Oregon	.25	- .29 (.66)	- .19 (2.53)	—	.30
Pennsylvania	.08	.54 (11.12)	-1.00 (62.80)	.0024 (5.16)	.93
Rhode Island	.08	.29 (3.76)	- .33 (3.81)	—	.53
South Carolina	.68	.03 (.12)	- .89 (.13)	—	.02
South Dakota	- .23	.78 (2.78)	- .39 (2.79)	—	.37
Tennessee	.05	.37 (5.50)	- .35 (8.74)	—	.54
Texas	- .43	.14 (3.66)	.36 (9.55)	—	.76
Utah	- .11	1.03 (29.11)	- .67 (19.18)	—	.81
Vermont	- .26	.15 (2.65)	- .43 (7.76)	.0052 (7.67)	.63
Washington	.33	.63 (2.91)	-1.13 (21.93)	—	.77
West Virginia	- .14	1.12 (18.66)	- .58 (9.55)	—	.77
Wisconsin	- .08	.37 (2.58)	- .21 (6.76)	—	.54
Wyoming	.01	.79 (6.58)	- .87 (7.43)	—	.66

Note: The coefficients are unstandardized regression coefficients. The numbers in parentheses are *F*-values.

The evidence supports our hypothesis. There is a strong tendency for the president's party to lose seats at the midterm. A histogram of gains and losses for the president's party in midterm elections is presented as Figure 2. In more than 3 out of 4 (308 of 402) state midterm elections examined, the president's party suffered a net loss of at least 1% of the seats. The president's party made gains in only 13% of the midterm elections, and no change was observed in the remaining 11%. Put

Figure 2. Net Change in the Percentage of State Legislative Seats for the President's Party Following Midterm Elections



Percentage Point Seat Change for President's Party

somewhat differently, the president's party is more than six times as likely to lose seats as it is to win seats. The mean seat change in all midterms examined was a 7.3% loss for the president's party. Of course, as in the case of seat losses in presidential elections, this level of analysis is a pretty rough cut of the data. The repercussions of presidential coattails for midterm elections are better examined in a less aggregated and multivariate analysis.

The single-equation model of midterm seat loss is quite similar to the coattail model for presidential elections. The dependent variable is a simple change variable computed by subtracting the percentage of state legislative seats held by Democrats after the presidential election from the percentage held after the midterm election. The independent variables in the midterm equation are identical to those used in the presidential election equation: two major independent variables, and a third independent variable introduced to control for trends favoring either of the parties. As in the presidential election equation, the first independent variable is the percentage of the presidential vote won by the Democrats. There is, however, one very important difference from the presidential election analysis: Whereas the effect of the presidential vote on seat changes for a party is hypothesized and shown to be positive in presidential elections, it is hypothesized to be negative in midterm elections. State legislative candidates receiving more help from the top of the ticket in presidential years have more help to lose in the following midterm election.

The second independent variable is the same Democratic base variable used in the presidential election equation, the percentage of seats held by the Democratic party prior to the last presidential election. The selection of this variable posed a problem. The logical choice of a base variable for the midterm equation is the percentage of seats held prior to the midterm, rather than the percentage held prior to the previous presidential election. However, because seat gains at the presidential election are dependent on the presidential vote, the base variable following the presidential election is quite often highly correlated with the presidential vote. The collinearity problem, particularly when we are dealing with so few cases, can be substantial, and the coefficient estimates are often unstable or insignificant as a result. For this reason, the pre-presidential election base is substituted for the post-presidential election base in the regressions.[3]

The third independent variable is a trend variable identical to that used in the presidential election equation. It is simply a counter variable entered to take trends over this period into account. Regressions including the trend variable are only reported when it is statistically significant. Otherwise the regressions do not include the trend control.

Table 2 presents regression results for the midterm model. The model accounts for a good bit of the variance in midterm seat change, though it generally performed less well than the presidential election equation. The equation accounts for 49% of the variance in the median state. One reason for the somewhat lower predictive power of this equation is the use of the pre-presidential base instead of the post-presidential election measure. Using the post-presidential base, the equation accounts for 58% of the variance in the median state, and for more than half of the variance in 27 of the 41 states. This performance is on the same order as that of the presidential election equation.

The regressions indicate that midterm seat changes are in fact a negative function of the previous presidential vote in the state. Parties lose seats at the midterm in an inverse proportion to how well their presidential candidate did in the state in the previous election. The withdrawal of presidential coattails significantly affects midterm results. The pattern is quite consistent. The coefficient of the presidential vote is negative in 39 of the 41 cases. It is also statistically significant ($p < .05$) in half of the cases (21 of the 41), despite the fact that a variety of considerations having the possibility of distorting the relationships could not be entered into the equation because of the limited number of cases.

As with the initial coattail effects, the strength of midterm coattail repercussions varies considerably. The latter effects range from the slightly positive coefficients found in North Carolina and Vermont to coefficients of -1.0 or less in 18 different states. The coefficients are less than -2 in 5 states. The median coefficient for the presidential vote is -.67. When the post-presidential election base is used, the presidential vote coefficients are slightly smaller. The median presidential vote coefficient in these regressions is -.52.

As in the case of presidential-related gains in presidential elections, presidential-related losses at the midterm can be set in some context by comparison to congressional election patterns. In midterm elections the president's party loses about 3.2 seats in the House of Representatives for every additional percentage point won by the president in the prior presidential election (Campbell, 1985). In percentage terms, a loss of 3.2 seats out of 435 seats translates into a coefficient of -.74, just slightly more negative than the median coefficient for coattail removal losses in state legislative midterms.

A second point of comparison for presidential-related midterm losses is, of course, the presidential coattail effect of the prior election. The negative presidential vote coefficients in midterm elections are of about the same magnitude as the positive presidential vote coefficients in presidential election years. The symmetry

Table 2. The Coattail Repercussion Model of Change in State Legislative Seats in Midterm Elections, 1946–1982

State	Intercept	Presidential Vote	Initial Base	Trend	R^2
Alaska	.52	-1.04 (1.64)	-.13 (.04)	—	.36
Arizona	.25	-1.10 (14.05)	.33 (7.00)	—	.68
Arkansas	.09	-.02 (.22)	-.78 (.06)	—	.05
California	.47	-.67 (4.67)	-.28 (1.93)	—	.51
Colorado	.68	-1.48 (10.49)	-.19 (.51)	—	.60
Connecticut	-.53	-.39 (.36)	-1.31 (3.37)	.0231 (3.70)	.41
Delaware	-1.33	-2.86 (9.89)	.16 (.02)	—	.59
Florida	.12	-.30 (.88)	-.86 (0)	—	.19
Georgia	.07	-.05 (7.77)	-.04 (.42)	—	.66
Hawaii	.22	-.16 (.21)	-.16 (.06)	—	.13
Idaho	.30	-.55 (2.89)	-.16 (.19)	—	.37
Illinois	.65	-1.53 (74.45)	.12 (.23)	—	.91
Indiana	1.50	-3.54 (14.51)	.18 (.38)	—	.71
Iowa	1.23	-2.76 (151.05)	-.03 (.07)	—	.96
Kansas	.35	-.62 (6.33)	-.30 (4.11)	—	.59
Maine	.25	-.54 (7.64)	0 (0)	—	.52
Massachusetts	.07	-.13 (2.60)	.02 (.14)	—	.28
Michigan	.36	-.95 (3.45)	.14 (.42)	—	.37
Missouri	.26	-.63 (2.18)	.12 (.26)	—	.36
Montana	.92	-1.65 (49.72)	.08 (.29)	-.0034 (3.98)	.91
Nevada	.24	-.28 (.58)	-.26 (1.60)	—	.24
New Hampshire	.37	-.30 (14.28)	-1.22 (14.72)	.0035 (11.52)	.86

TABLE 2 (continued)

State	Intercept	Presidential Vote	Initial Base	Trend	R^2
New Mexico	1.53	-1.62 (5.48)	.13 (.13)	-.0128 (7.69)	.60
New York	.11	- .51 (13.30)	- .38 (2.53)	.0051 (6.90)	.79
North Carolina	.16	- .52 (3.22)	.13 (.05)	—	.41
North Dakota	.77	-1.77 (9.77)	- .14 (.24)	—	.63
Ohio	.56	-1.05 (2.96)	- .11 (.19)	—	.30
Oklahoma	.28	- .05 (.12)	- .32 (.96)	—	.14
Oregon	.51	-1.06 (7.55)	- .02 (.03)	—	.53
Pennsylvania	.25	- .85 (2.68)	.30 (.96)	—	.34
Rhode Island	.23	- .50 (10.91)	.09 (.22)	—	.62
South Carolina	- .07	.02 (.04)	.04 (.03)	—	.01
South Dakota	.49	-1.05 (4.10)	- .14 (.31)	—	.41
Tennessee	.23	- .43 (2.29)	- .03 (.02)	—	.33
Texas	.65	- .11 (3.90)	- .45 (4.73)	-.0026 (5.60)	.49
Utah	2.22	-2.43 (17.72)	- .49 (2.68)	-.0169 (7.67)	.80
Vermont	- .03	.17 (1.27)	- .06 (.29)	—	.17
Washington	.90	-1.51 (11.00)	- .39 (1.80)	—	.63
West Virginia	1.00	-2.10 (57.94)	- .17 (.63)	—	.89
Wisconsin	.53	-1.03 (6.38)	- .02 (.02)	—	.48
Wyoming	.45	-1.08 (5.89)	0 (0)	—	.46

Note: The coefficients are unstandardized regression coefficients. The numbers in parentheses are *F*-values.

can be seen in a comparison of median coefficients, but is even clearer in comparisons of individual state coefficients. Where we find evidence of strong coattail effects in presidential election years, we tend to find evidence of equally strong coattail repercussions at the midterm. Conversely, where we find evidence of weaker coattails, we also tend to find weaker coattail removal or repercussion effects. A more systematic comparison indicates how closely coattail gains and subsequent coattail losses are related. The presidential vote coefficients from Tables 1 and 2 are themselves strongly correlated with one another ($r = -.80$). One may interpret this negative association to mean that presidential coattails offer little or no residual benefits for a party. The benefits in the presidential election last only until politics returns to normal at the midterm.

As in the presidential election regressions, the base, the second of the principal independent variables, had a negative effect on seat change. As expected, however, the effect was somewhat stronger and more consistently negative when the post-presidential election base was used. The pre-presidential election base had a negative effect in 37 of the 41 regressions and was statistically significant ($p < .05$) in eight cases. Recall that the pre-presidential base has been used in Table 2 to reduce collinearity with the presidential vote variable.

Variations Among the States

The effects and midterm repercussions of presidential coattails on state legislative outcomes, though evident in most states, varies a good bit from state to state. Given the strong correlation ($r = -.80$) between the coefficients for the presidential year and the midterm, there is good reason to believe the state-to-state variation is systematic. The source of this systematic variation is somewhat less clear. There are a number of possibilities. Particularly heterogeneous states may be less uniform in their support for a presidential candidate, and consequently appear less responsive to the average vote in the state for that candidate. The very weak effects found in California, for instance, may be traceable to the very heterogeneous nature of the state. The varying sizes of state legislative districts, the use of multimember districts, and the drawing of district boundaries may dampen or accentuate coattail effects, particularly as they vary within a state over four decades. The popularity of the president at the midterm can certainly accentuate or depress repercussion effects. Secular trends in a state, untapped by the crude trend indicator in the equations, may also complicate matters. Large scale immigration into western states over this period may be one such trend. Of the many potential sources of interstate variation, two in particular will be examined: interparty competition and ballot form.

Party Competition

States with competitive party systems should respond more strongly to fluctuations in presidential politics than states dominated by a single party. No presidential candidate, no matter how popular, can help his party capture state legislative seats unless his party actively contests those seats. Two measures of party competition are used to test the proposition of the relationship of party competition to coattail effects.[4] The first measure is based on the mean percentage of state legislative seats held by a party over the period from 1944 to 1982. This competitiveness index ranges from a value of 100 in states where the parties evenly split the state legislature to a value of 50 in states completely dominated by a single party. The second party competition measure taps the volatility of state legislative elections. This second measure is the standard deviation of the distribution of state legislative seats in all elections in this period.

According to both measures of party competition, there is a relationship between coattail effects, both presidential (Table 1) and midterm (Table 2), and state party competition. Both measures are positively correlated with coattail coefficients in presidential elections ($r = .31$ and $r = .34$, respectively) and negatively correlated with coattail repercussion coefficients in midterm elections ($r = -.46$ and $r = -.45$, respectively). An inspection of the scatterplots of both coefficients against the party competition measures reveals a bit more about the relationship. The plots suggest substantial heteroscedasticity. Coefficients vary a good bit among competitive states but are uniformly small in noncompetitive states. The 7 least competitive states of the 41 examined are Arkansas, Florida, Georgia, North Carolina, Oklahoma, South Carolina, and Texas. Each had a score of .7 or less on the mean percentage indicator of party competition. Only one of these 7 noncompetitive states (North Carolina, .49) had a coattail coefficient greater than .25, and only 1 (again North Carolina, −.52) had a coattail repercussion effect more negative than −.30. The range of coefficients among the remaining more competitive states is substantial. Among the 15 most competitive states—those having a score of .9 or more on the mean percentage indicator of party competition—coattail effects ranged from a high of 2.75 in Indiana to a low of −.29 in Oregon. Coattail repercussion coefficients were similarly dispersed, ranging from a strong effect of −3.54 in Indiana to a weak effect of −.39 in Connecticut. In other words, party competition is a necessary but not a sufficient condition for strong coattail effects. In competitive states a number of factors previously mentioned (e.g., heterogeneity) may mask the coattail effects in individual state legislative district contests.

Ballot Form

Coattail effects ought to be stronger in states with ballot provisions for a straight party vote (Weber and Parent, 1985, p. 26). Generally, such a provision is included in the party-column ballot, but not all forms of the party-column ballot have the single mark option for the straight party vote. On the basis of nine reports in the Council of State Governments' *Book of the States* between 1943 and 1982, the 41 states in this study were grouped by their provision for a straight party vote. Fourteen of the 41 states changed their straight party vote provisions during this period, and were consequently dropped from this portion of the analysis. An additional 3 states were dropped because they were noncompetitive as measured by the first party competition measure (a score of 70 or less). The remaining 24 states divided nearly evenly, 11 having no straight party voting provision, and 13 having the option.

As expected, coattail effects appear somewhat stronger in the states with the straight party vote option. The median coattail effect in states lacking a straight party vote ballot is .49. The median coattail effect coefficient in states with a straight party vote ballot is .83. An analysis of variance, however, indicates that the difference is not significant at the .05 level ($p = .17$).

Gubernatorial Coattails

As portrayed to this point, the dynamics of state legislative elections would appear to be entirely driven by presidential electoral politics. Of course this is not the full story. Local politics undoubtedly have some effect. Caldeira and Patterson (1982b) have shown that local campaigns have a significant influence on state legislative election results. Moreover, candidates other than those running for president, most notably gubernatorial candidates, may have coattails (Weber, 1980). To test further the reliability of the presidential coattail findings and to set those findings in context, gubernatorial coattails are estimated by inserting the Democratic gubernatorial two-party vote percentage in the presidential and midterm equations of state legislative seat change. The equations are estimated for the 8 states consistently holding gubernatorial elections throughout this period in presidential election years, and for the 30 states consistently electing their governors in midterm elections. Three states (Florida, Illinois, and North Dakota) are eliminated because of their changes in the scheduling of gubernatorial elections. Also, because of the small number of cases per state, the trend variable is omitted. The regression estimates are presented in Table 3.

These regression estimates make three points relevant to our analysis. First, the regression results generally indicate that candidates for governor have coattails that extend to their parties' state legislative candidates. Positive gubernatorial effects were found in 3 out of 4 states (29 of 38). The median gubernatorial effect is .37; that is, a party should gain about 2% of the state legislative seats for every 5 additional percentage points won by its candidate for governor. While this effect is statistically significant in a minority of states (8 of 38)—perhaps because of the small number of cases—it would appear that a strong run by a party's candidate for governor helps the party win seats in the state legislature.[5]

Second, the previous estimates of presidential coattail effects and their repercussions withstand the introduction of gubernatorial coattails. The coefficients still fit the expected pattern of positive effects in presidential elections and negative effects in midterm years. The expected signs are found in more than 9 out of 10 states (35 of 38). It is true that there are significant differences between the coefficients for a few states shown in Table 3 and the comparable coefficients that were estimated without controlling for gubernatorial coattails. However, in 3 out of 4 states, the difference between the estimates is less than .3, and the two sets of estimates are quite highly correlated (r = .90 in presidential year cases, and r = .83 in midterm year cases). Moreover, the estimated coefficients in Table 3 are nearly as likely to be stronger than the previous estimate (16 v. 22) as they are to be weaker.

The final point to draw from these regressions concerns the relative strength of presidential coattails and gubernatorial coattails. How do the presidential effects measure up against the gubernatorial effects on state legislative contests? The regressions suggest that the two effects are roughly equal in strength. The basis of this conclusion is a comparison of the absolute values of the standardized regression coefficients for the two variables in each of the 38 regressions. In the 8 states holding concurrent presidential and gubernatorial elections, presidential coattails appeared a bit stronger than gubernatorial coattails. Presidential effects were clearly stronger in 5 of the states, and very small differences (less than .05) were found in the remaining 3 states. In the states electing governors in midterm elections, half exhibited stronger gubernatorial coattails than presidential coattail repercussion effects, and half exhibited stronger presidential coattail repercussion effects than gubernatorial coattails.

Conclusion

There is a pulse to state legislative elections, a regular pattern of gains and losses very much like that observed in congressional elections. Moreover, the pulse is regulated by presidential electoral politics, again much like that of congressional elections. Presidential coattails extend to state legislative candidates. All things being equal, the change in a party's share of state legislative seats in presidential election years is proportional to the share of the vote won by its presidential candidate in the state. In presidential election years, a presidential candidate running a strong race in a state helps his party to gain additional state legislative seats. The opposite pattern is found in midterm elections. Whatever help the presidential candidate extended to his party's state legislative candidates in presidential election years is absent in the midterm election. Parties initially helped to significant

Table 3. Gubernatorial Coattail Effects on Changes in State Legislative Seats, in Presidential and Midterm Elections, 1944–1982

State	Intercept	Presidential Vote	Initial Base	Gubernatorial Vote	R^2
		Presidential Elections			
Delaware	-.44	1.08	-.56*	.43	.66
		.50	-.71	.22	
Indiana	-.92	1.64*	-.97	1.24	.89
		.36	-.68	.20	
Missouri	-.16	.67	-.45	.21	.74
		.43	-.53	.10	
Montana	-.35	.70	-.99*	1.02	.74
		.36	-.70	.37	
North Carolina	.32	.37	-.65	.07	.56
		.89	-.76	.12	
Utah	-.18	1.05*	-.66*	.12	.82
		.85	-.65	.09	
Washington	-.05	.54	-1.28*	1.05	.83
		.25	-.91	.27	
West Virginia	-.37	.69*	-.74*	1.04*	.89
		.47	-.65	.51	
		Midterm Elections			
Alaska	.34	-1.12	-.06	.38	.39
		-.67	-.05	.20	
Arizona	.06	-1.11*	.34*	.36	.76
		-1.49	1.08	.29	
Arkansas	.05	-.04	-.07	.06	.30
		-.36	-.09	.54	
California	.26	-.28	-.46*	.29	.72
		-.24	-.62	.61	
Colorado	.88	-1.64*	-.20	-.25	.61
		-.87	-.19	-.12	
Connecticut	-1.08	-.04	-.33	2.59*	.76
		-.02	-.36	.93	
Georgia	.07	-.04	-.03	-.01	.66
		-.66	-.15	-.09	
Hawaii	1.72	.01	-1.17	-1.65	.44
		.12	-1.04	-.97	
Idaho	.45	-.68	-.15	-.20	.39
		-.67	-.13	-.18	
Iowa	1.40	-2.73*	-.03	-.40	.97
		-.96	-.03	-.11	
Kansas	.02	-.65*	-.42*	.76	.75
		-.64	-.70	.46	
Maine	.35	-.55*	.02	-.22	.56
		-.74	.05	-.20	

Table 3 (continued)

State	Intercept	Presidential Vote	Initial Base	Gubernatorial Vote	R^2
Massachusetts	.15	-.19	.03	-.12	.31
		-.77	.18	-.32	
Michigan	-.81	-.12	.17	1.60*	.68
		-.07	.25	.74	
Nevada	-.20	-.18	-.33*	.83*	.80
		-.16	-.52	.76	
New Hampshire	.15	-.40*	-.37	.34	.76
		-.83	-.28	.42	
New Mexico	-.88	-.28	.13	1.94	.16
		-.13	.10	.32	
New York	-.10	-.30	.02	.53	.65
		-.41	.03	.48	
Ohio	-.48	-.18	.02	1.19	.44
		-.10	.02	.59	
Oklahoma	-.03	.07	-.31	.45*	.76
		.17	-.33	.84	
Oregon	.27	-.98*	-.11	.55	.64
		-.66	-.21	.38	
Pennsylvania	-.86	-.24	.18	1.80*	.70
		-.15	.18	.71	
Rhode Island	.22	-.49	.08	.02	.62
		-.75	.10	.04	
South Carolina	.24	-.04	-.40	.18	.34
		-.17	-.59	.93	
South Dakota	.20	-1.30*	-.22	.96*	.76
		-.74	-.26	.62	
Tennessee	.22	-.44	.11	-.14	.36
		-.57	.18	-.28	
Texas	.14	-.07	-.20*	.19*	.73
		-.52	-.67	1.21	
Vermont	-.10	.14	-.09	.10	.42
		.33	-.29	.52	
Wisconsin	-.25	-.52	-.20	1.24	.66
		-.35	-.39	.64	
Wyoming	.85	-1.30*	-.12	-.54	.50
		-.81	-.08	-.26	

Note: The top coefficients are unstandardized regression coefficients, and the bottom coefficients are standardized regression coefficients.

$^{*}p < .05$.

gains subsequently suffer commensurate losses when running without the benefit of coattails. In midterms, all things being equal, the change in a party's share of state legislative seats is inversely proportional to the share of the vote won by its presidential candidate in the prior presidential election in the state.

The evidence of a coattail pulse in state legislative elections is quite strong. First, the pattern is consistently found in different states. In 39 of 41 state-level presidential election year multivariate regressions, the presidential vote has the expected positive coattail coefficient. Again in 39 of 41 state-level midterm election year multivariate regressions, the presidential vote has the expected negative coattail repercussion coefficient. Second, the pattern is statistically significant in a majority of states, despite the small number of cases (elections) considered. Third, the median magnitude of presidential coattail and repercussion effects in state legislative races is only slightly less than those effects in congressional races. Fourth, though the effects varied from state to state, this variation appears to be systematic. Presidential coattail repercussion effects in a state are generally comparable to the initial presidential coattail effects. State circumstances that blunt aggregate coattail effects, such as ballot forms and the lack of two-party competition, also blunt aggregate coattail repercussion effects. The correspondence of the two types of effects should add to our confidence about the findings. Where evidence is found of one of the effects, there should be evidence of the other, and this is precisely what we find. Finally, presidential coattail and repercussion effects continue to be found after controls for gubernatorial coattail effects are introduced.

Having carefully documented the substantial role presidential candidates play, by their presence in presidential election years and their absence in midterm election years, in determining state legislative election outcomes, the natural question is, what does this mean for state legislative politics? What are the more general political and policy implications? Are presidential coattails at the state legislative level beneficial or harmful to our politics?

The conventional wisdom regarding presidential coattails in congressional elections has been that the system benefits from the linkage of presidential and congressional electoral politics. Coattails promote cooperation between the executive and legislative institutions. Congressmen beholden to the president for their election or reelection, or perhaps even for their future reelection, have sympathetic ears for the president's proposals. If the institutions are structured to disperse power, possibly creating a chaotic or deadlocked system (Burns, 1967), then coattails are a compensating force promoting leadership and cooperation.

In state legislative politics, the role of presidential coattails is not quite so clear. On the face of it, one might conclude that presidential coattails in state legislative elections are detrimental to the system, or at best introduce extraneous noise into state politics. The problems facing the national government are not those facing state governments, which also possess different powers. According to this assessment, there is no reasonable linkage of national and state politics by presidential coattails.

However, the case can be made that presidential coattails are desirable in state legislative elections for much the same reasons they are desirable in congressional elections. To be sure, national and state issues often differ, but we can easily underestimate the number of issues common to both national and state politics, and the cooperation and coordination required to address these overlapping concerns. In a sense, presidential coattails in state legislative elections may help bridge the federalism gap between national and state governments, in much the same way that presidential coattails in congressional elections may help bridge the separation of powers gap between the president and Congress. Moreover, even when the national and state governments are not dealing with common issues, the public may benefit from its different governments pursuing different policy areas with some common underlying perspective or ideology. Presidential coattails, at whatever level, may promote this common perspective. Presidential coattails seem to foster greater coherence between policies of nation and state, even though such bonding between them presumably is smaller than could be effected by "responsible" political parties.

Notes

I would like to thank John Bibby, Stephanie Greco Larson, and Jeff Stonecash for their comments on earlier versions of this article, and Kevin Lasher for his help in gathering the data. An earlier version of this article was presented at the 1984 meeting of the Southern Political Science Association.

1. For an excellent discussion of the different constructions of election outcome variables in state and local election research, see Weber and Parent (1985, pp. 3–9). The advantage in using a seat-change variable as the dependent variable to construct the necessarily parsimonious model required by the small number of cases per state is that a number of variables that might affect the absolute success of a party (e.g., partisanship in the state) are likely to be near constant over a two-year period, and would not affect change in a party's success.

2. If there were no coattail effects, the probability of estimating a positive coefficient in any given state would be approximately one-half. Given this probability, a binominal distribution indicates that there is less than one chance in one thousand of estimating 39 or more positive coefficients out of 41 states. Thus, the proposition that coattail effects do not exist at this level can be quite safely rejected.

3. Results of regressions using the post-presidential election base rather than the pre-presidential election base are available from the author.

4. There are a variety of choices in selecting indicators of party competition. The two indicators used here are selected for several reasons. First, they cover the entire period under study. Although levels of party competition in a state may change over time (Broh and Levine, 1978; Patterson and Caldeira, 1984; Tucker, 1982), the trend variable in the original regressions should correct for this movement. Second, the dispersion indicator of party competition is somewhat sensitive to the aggregation problem raised by Ray and Havik (1981). A state legislature might be evenly divided between the parties but lack competitive state legislative districts. By the mean percentage of seats indicator such a state would look more competitive than it really is. While the dispersion indicator does not directly measure this district-level competition, it should be somewhat sensitive to it. Third, the mean percentage of seats indicator is consistent with previous research in party competition, and focuses on competition for the particular office in question. Among others, Ranney (1976) and Patterson and Caldeira (1984) use a party's percentage of seats in the state legislature as one component of their more general indices of state party competition.

5. The gubernatorial coattail effects found in both presidential and midterm elections most probably have their own repercussion effects in the following election.

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THE POWER OF THE STATES IN U.S. PRESIDENTIAL ELECTIONS

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The Electoral College is a uniquely American political institution, yet its impact on both the power of the American states and the relative power of citizens living in different states is not well understood. Game theorists have broached the state power problem exclusively in terms of the size of each of the states. Empirical investigators have been less systematic, basing their analyses solely on which states have been close in a single election. In this paper we present a model of state power which combines the idea of the pivotal player from game theory with an empirical model of state voting. In doing so we provide a theoretically derived and empirically meaningful assessment of state power in presidential elections. We then trace the implications of the power of the states for the relative power of individual voters, finding large disparities between voters from different states.

How powerful is each of the American states in determining the winner in a presidential election? This question would be of no interest if citizens elected the president directly, rather than through the Electoral College. As the institutional mechanism used to choose presidents, the Electoral College has been and continues to be the object of debate. However, this debate has not been informed by knowledge of the actual impact of the Electoral College system on the relative power of the states, or on voters who live in different states. In this paper we shall provide a theoretically meaningful assessment of state power in presidential elections. We shall then trace the implications of our model for the one-man one-vote principle of importance to democratic political theory.

Previous attempts to assess the power of the states and of individual voters have essentially followed game theory (Banzhaf, 1968; Mann and Shapley, 1962; Margolis, 1977, 1983; Owen, 1975; Riker and Shapley, 1968). These analyses have relied only on the size of states without any consideration of their political competitiveness. Attempts by empirically oriented researchers to utilize competitiveness in assessments of state and group influence have been largely *ad hoc*, simply examining which states were close to the 50-50 split in a single election (for

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

Table 1. Illustration of the Calculation of the Shapley Value

Permutations[a]			
* ABCD	* BACD	* CABD	* DABC
* ABDC	* BADC	* CADB	* DACB
* ACBD	* BCAD	* CBAD	* DBAC
* ACDB	* BCDA	* CBDA	* DBCA
* ADBC	* BDAC	* CDAB	* DCAB
* ADCB	* BDCA	* CDBA	* DCBA
Column Totals			
0A	1A	1A	2A
1B	0B	1B	2B
1C	1C	0C	2C
4D	4D	4D	0D
Totals			
4A			
4B			
4C			
12D			

[a] Assume A has 3, B and C 2, and D 4 votes; * denotes pivot position.

example, Spilerman and Dickens, 1974).[1] We shall construct a model of state power which will take advantage of the analytic rigor of the game theory approach, and at the same time will recognize that states have different levels of electoral competitiveness.

The most widely used measure of power in weighted voting situations like the Electoral College is the Shapley value. The Shapley value defines the power of actor A as the number of permutations (orderings) in which A occupies the pivotal position (that is, orderings in which A can cast the deciding vote) divided by the total number of possible permutations. The idea can be illustrated with a simple example. Let us consider four states and label the states A, B, C, and D, with A having three votes, B and C having two votes each, and D having four votes. To calculate the traditional Shapley value we would consider the 4! = 24 possible orderings of the four states, and note each time a state plays the pivotal role of turning a coalition from a losing one into a winning one. For example, if the order from most Democratic to most Republican were CABD, B would be the pivotal state, since six votes are needed to win and C and A together account for 3 + 2 = 5 votes, and by including B's two votes they have a winning coalition.

Table 1 shows the 24 possible orderings of the states, and places an asterisk (*) above the pivotal state in each case. In this particular example the most powerful actor is D, playing the pivotal role one-half of the time, with A, B, and C having equal power, each playing the pivotal role one-sixth of the time. Notice that A's extra vote does not add to A's power, since A requires both B and C as partners, or D as a partner, which is similar to B and C's situations. Thus, having three votes gives A no more power than the two votes that both B and C have. In contrast, D's extra vote is quite potent. By calculating the Shapley value we have a far more meaningful assessment of state power than we did simply by knowing the basic 3-2-2-4 vote distribution: D is distinctly more powerful than B or C, while A is not.

The problem with the standard Shapley value is the untenable assumption that each ordering is equally likely. Suppose, going back to the four state example, that states A and B are heavily Democratic and ideologically liberal, state D is Republican and ideologically conservative, while state C is balanced between the parties and is ideologically moderate. Consistent with this, suppose that in a series of presidential elections the only orders from most Democratic to most Republican of the four states were BACD and ABCD. In this instance, with the same 3-2-2-4 vote distribution, state C would be the true power source, since C would always determine the winner. In the series of elections there might be some landslides where all four states voted for the same candidate, but there could not be a single election where C is on the losing side. Winning in state C, under these assumptions, is tantamount to winning the election. A power model which assigns half of the power to D and only one-sixth of the power to C, as the standard Shapley value does, is quite misleading and lacks verisimilitude.

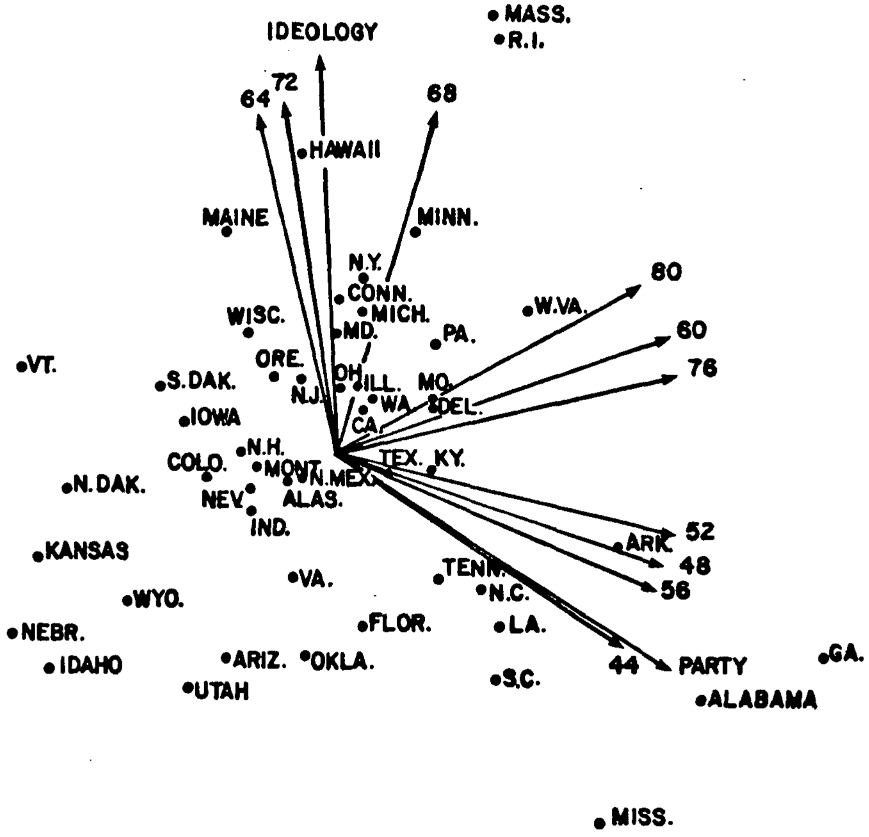
The pivotal player concept underlying the Shapley value can be applied to situations where all orderings of states are not equally likely to occur. Based on a suggestion of Owens (1971), Shapley (1977) himself has proposed such a modification. The idea is to observe the percentage of times each player occupies the pivotal location, now recognizing that the orderings are constrained by the political preferences (spatial locations) of the players. Using this modification in our example, C would be assigned a power value of 1.0, because it is always in the pivotal position, while each of the other players would be assigned a zero. This simple example serves to illustrate that any meaningful assessment of voting power must recognize that some orderings of states are more likely to occur than others.

The Structure of State Voting in Presidential Elections

The question of which of the many possible orderings of states are likely to occur is intrinsically a structural question.[2] We constructed the four state example above to be essentially unidimensional. The actual structure of states in a political space is likely to be more complicated than that. Unlike the example in which each state's partisanship matches its ideology, some states tend to be quite Democratic and at the same time quite conservative (e.g., Georgia), while others tend to be Republican and relatively liberal (e.g., Maine). In fact, we find when we analyze state voting in presidential elections that the structure of states in a political space tends to be two-dimensional, with most contemporary (post-1940) elections conforming well to that two-dimensional structure.[3]

The structural (spatial) analysis of states that we perform uses principal components to analyze the Democratic percentage of the presidential vote in the 50 states from 1944 to 1980, where each election year is a variable and the 50 states are the units of analysis. In Appendix 1 we

Figure 1. Fifty States and Elections with External Axes



show that based on the individual-level spatial choice model, this is an appropriate method to estimate the location of state means in a political space, as well as to estimate the location of election vectors (axes) in that space.[4] The spatial positions of the states along with the orientations of the election vectors are shown in Figure 1. We have also included on the figure two reference axes, one of which is designed to measure the relative partisanship of the states, and the other the relative ideology of the states. They were placed in the space subsequent to the factor analysis, and have exerted no influence whatsoever on the structure. The reference axes are included to facilitate interpretation of the structure.[5] We selected the 1944–1980 set of elections because the time frame is politically coherent, postdating both the great depression and the start of U.S. involvement in the Second World War, and is long enough to provide some historical scope.

The state locations appear consistent with an interpretation of the space according to party and ideology. The upper left quadrant of the space is the liberal Republican section, with no state occupying an extreme position in this quadrant. Maine, and to a lesser extent Wisconsin, are the most obvious members of this group. The liberal Democratic section of the space is in the upper right, with Massachusetts and Rhode Island the salient members of that group. The southern states lie primarily in the lower right, conservative Democratic quadrant of the space. Several of the mountain and traditionally Republican midwestern farm states occupy the lower left, with Nebraska and Idaho in the most extreme conservative Republican position. Partisanship was of great importance in the elections from 1944 through 1956. The 1964, 1968, and 1972 elections are more ideological, while 1960, 1976, and 1980 present a mixture of party and ideology. In short, the structure seems intuitively reasonable, and the general orientation of the elections fits nicely with analyses of the specific elections based on mass national surveys.[6]

The spatial model which underlies the factor analysis is a *long-term relative support model.* The model conceives of the ordering of support for the two major party candidates in each state as a function of the position of the state on the two critical underlying dimensions (party and ideology). The model is not concerned with predicting outcomes in the sense of winners and losers; hence it does not consider the variety of short-term forces (candidate appeal and national economic conditions, for example) which are likely to have a consistent impact on the vote in each of the states. The concern is with relative support rather than actual voting percentages, because it is the position of a state in the order from most Democratic to most Republican which determines whether or not that state occupies the pivotal position. Electoral tides may make an election overwhelmingly Democratic or Republican, but the state in the pivotal position is always the fulcrum that divides the states into those which are relatively Democratic and those which are relatively Republican. In a sufficiently close election it is the pivotal state which determines the outcome.

The structural model is not a perfect predictor of electoral orderings. Such factors as local economic conditions, home state effects, and transient issues of particular concern to a state or region are likely to influence the order of state support in any specific election. These state-specific effects are most naturally modelled as random perturbations, and our model of state power will incorporate a random component to represent these forces. Of more immediate concern is the question of how well the structural model accounts for the behavior of the states. The structural model predicts voting patterns over a four decade period on the basis of just two underlying explanatory dimensions. Quite conceivably the model could account for very little. That is not the case.

The structure recovered is quite a good predictor of the specific elections. The results of the principal component analysis are summarized in Tables 2 and 3. Two components account for 79.2% of the variance in state voting across the elections. The first component accounts for 48.1% and the second 31.1% of the total variance in election results. If a third component were added, relatively little additional variance (6.5%) would be explained. With the exception of 1948, every election has at least 70% of the variance accounted for by the component analysis. In 1948 a substantial 57.1% of the variance is still explained.

The structure displayed in this component analysis is critical to the development of a model of state power. If there were no structure undergirding the voting behavior of states, we would be back to the original Shapley assumption that each potential ordering of states is equally likely. The high degree of structure shown in the spatial analysis suggests that some orderings are far more likely to occur than others. Furthermore, the structure forms the basis for assessing how likely various orderings are to occur. The order in which the states are predicted to vote in an elec-

Table 2. Summary of Principal Component Analysis

Component	Eigenvalue	Percent Variance Explained	Cumulative Percent Variance Explained
1	4.81	48.1	48.1
2	3.11	31.1	79.2
3	0.65	6.5	85.7
4	0.52	5.2	90.9
5	0.34	3.4	94.3
6	0.21	2.1	96.3
7	0.17	1.7	98.0
8	0.08	0.8	98.8
9	0.07	0.7	99.6
10	0.04	0.4	100.0

tion is the order in which they project on the election axis. This follows directly from the linear nature of the model underlying factor analysis and the proof we show in Appendix 1.

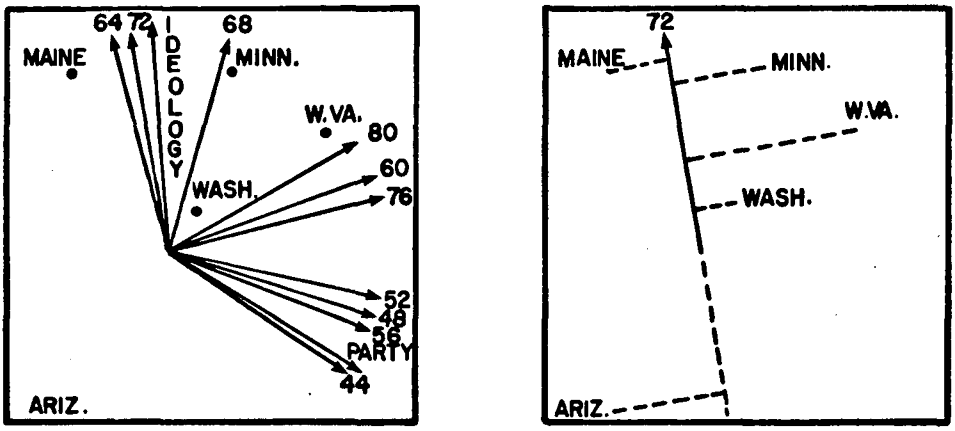
We can readily illustrate how the orderings are obtained. Consider the spatial position of just the five states (to keep the figure uncluttered) that appear in Figure 2. Panel A of the figure shows the five states and the various election axes; it is the same as Figure 1, with five rather than fifty states. Panel B shows the projections of the five states on the 1972 election axis. The states project in the order Maine, Minnesota, West Virginia, Washington, and Arizona. In an election with the particular orientation of the 1972 election, the prediction is that the most Democratic of the five states would be Maine and the most Republican, Arizona. The orientation of the 1972 election axis is very much in the ideological direction, suggesting ideology was predominant in determining the order of the states in that year.

The orientation of the 1980 election axis is about midway between the reference party and ideology axes, suggesting each was about equally important in that election. The projection of the five states on the 1980 axis is shown in Panel C. The order is West Virginia, Minnesota, Washington, Maine, and Arizona—quite different from that of the states in the more

Table 3. Varimax Rotated Coefficients and Variable Communalities

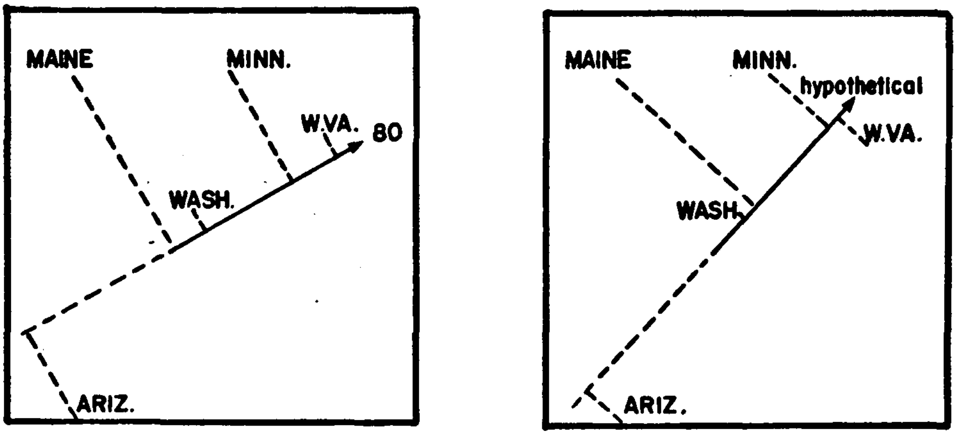
Year	Loadings on: Dimension 1	Loadings on: Dimension 2	Percent Variance Explained (100X Communality)
1944	0.76	−0.51	82.8
1948	0.72	−0.24	57.1
1952	0.94	−0.22	93.0
1956	0.83	−0.34	79.6
1960	0.83	0.29	76.8
1964	-0.20	0.88	81.0
1968	0.25	0.89	86.2
1972	-0.13	0.91	83.9
1976	0.84	0.20	74.8
1980	0.76	0.44	76.4

Figure 2. Projections of Five State Points on Several Axes


64
72
IDEOLOGY
68
MAINE
MINN.
W.VA.
80
60
76
WASH.
52
48
56
PARTY
44
ARIZ.
72
MAINE
MINN.
W.VA.
WASH.
ARIZ.

Panel A
Configuration of Points and Axes

Panel B
Projections on 1972 Axis


MAINE
MINN.
W.VA.
80
WASH.
ARIZ.
MAINE
MINN.
hypothetical
W.VA.
WASH.
ARIZ.

Panel C
Projections on 1980 Axis

Panel D
Projections on Hypothetical Axis

ideologically oriented election shown in Panel B. Panel D shows the projections of the states on an election axis we drew to represent a hypothetical election which is somewhat more ideologically oriented than the 1980 election. We drew this panel to make clear that any axis drawn in the space represents a hypothetical election and will yield a specific ordering of states. The ordering in this case is West Virginia, Minnesota, Maine, Washington, and Arizona.

The predicted and actual orders for the 1952, 1972, and 1980 elections appear in Appendix 2, and give some sense of the relationship between the actual election results and those recovered by the analysis. Each of the elections represents one of the three basic clusters that we will label Party, Ideology, and Mixed, respectively. The states that occupy both the predicted and actual pivot positions are italicized in the table. The state which occupies the actual pivot is also denoted by a "*" in the predicted column.

The predicted orders are quite accurate. The Spearman rho correlations between the predicted and observed orders are .979, .909, and .831 in the three cases, respectively. As should be expected, these closely parallel the communalities reported in Table 3 for the three elections. Of the three states which actually occupy the pivot position, Michigan is predicted to be two states away from the pivot in 1952, Missouri two states away from the pivot in 1972, and Illinois one state from the pivot in 1980. California is the predicted pivot in each case and actually falls on the Republican side of the pivot in 1952 and 1980, and on the Democratic side in 1972.

Upon further examination of those states that are close to the pivotal position, we do find some variation in the three types of elections. Massachusetts, for example, is close to the pivot in the party-oriented 1952 election, but quite far from the pivot in the 1972 and 1980 orientations. In contrast, the two Carolinas and Louisiana are closer to the pivot in the mixed 1980 orientation than in either the 1952 or 1972 orientation, while New Hampshire is closer to the pivot in the ideologically oriented 1972 election. These different orderings suggest that strategic importance is itself a function of election type. That the strategic importance of states will vary by election type makes considerable sense: different states are likely to be important in different kinds of elections. The fact that state power depends on election type, however, does complicate the analysis.

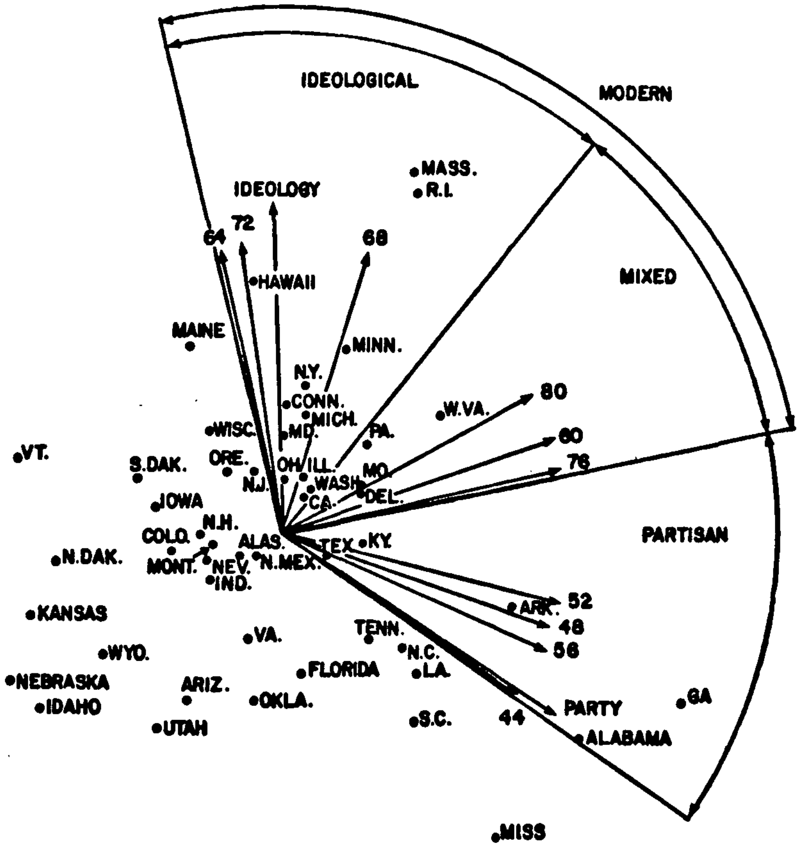
In Figure 3 we show the configuration recovered in the component analysis divided into ideological mixed, and partisan sectors. The ideological and mixed sectors are also combined to form a modern sector. We shall construct distinct power measures for each of the states in each of these sectors. The boundaries of the sectors are based on the cluster structure. The ideological sector begins one degree counterclockwise of the 1964 election axis, and goes to the midpoint between the 1968 and 1980 elections. The end of the mixed sector is one degree clockwise of the 1976 election. The modern sector is thus the sector which incorporates all the election orientations observed since 1960. The party sector ends one degree clockwise from the 1944 election. In general, the particular boundaries selected are not critical, since mild changes of boundaries have very little impact on power measurements.[7]

Judgments Underlying the Power Model

To assess power in a sector requires two judgments: (1) Where in the sector are future elections likely to be located? (2) How likely are the orderings of states in future elections to conform to those predicted from the spatial analysis? Without additional information it seems most appropriate to assume elections can occur anywhere in the sector with equal likelihood, i.e., a uniform distribution of elections in each sector.

The second judgment is more complex. There is always the possibility that future elections might not follow the structural constraints laid down by those in the past. This is less a danger than it might first appear. A model based on past presidential voting would have been off the mark in only one election in the 1944–1980 sequence—that of 1964. At the time of the 1964 election, and for several years preceding it, the ideological preferences of the states (as measured by the average ADA and ACA ratings of state congressional delegations) were quite different

Figure 3. Political Space Divided into Sectors



from the way the state electorates were voting in presidential elections. The apparent deviance of 1964 was simply the first presidential election in which the ideological dimension became influential in presidential voting. No such anomaly between the voting behavior of state congressional delegations and the structure of state voting in presidential elections currently exists. Hence, presidential elections in the near future are likely to be structurally similar to those within the recent past.

Once we recognize that past structure is a useful guide to the structure of future elections, we must consider how accurate the predictions are likely to be. The principal component analysis recovered 79.2% of the variance in outcomes in the 1944–1980 series of presidential elections, and only in the 1948 election did the variance explained drop below the 70% level. Principal component analysis as a technique, however, is *post hoc*, and does not necessarily constitute a reasonable means for assessing the structural predictability of future cases. Therefore, we performed a second principal component analysis based on just the 1944–1968 series of elections, and then regressed the 1972–1980 elections on the two recovered components. The variance explained in the three elections was 71.6%, 52.3%, and 52.4%, respectively, for an average explained variance of 58.8%. Since at least half the variance was explained by previous structure in these three cases, and the average was between 50-60%, we felt a reasonable yet conservative estimate of state power would allocate half the variance to previous structure, and half to election-specific factors not predictable prior to the campaign itself.

A Monte Carlo Simulation Approach

Once we have made the judgments concerning the distribution of orientations within the sectors of interest and the relative impact of past structure versus idiosyncratic effects, we can assess state power by simulating large numbers of elections within each sector. A simulation approach is dictated by the complexity of the problem, and parallels the original approach of Mann and Shapley (1960) to the simpler state power problem in which only state size was considered.

The Specific Method

Each orientation of a potential election axis provides an estimate of how the states will be ordered based on how they project on that axis. We assume that any election with that orientation will differ from those projections based on random factors. If we denote the set of projections as the vector P_o and the set of normal random perturbations as the vector N_i, then the simulated election, E_{oi}, is the sum of P_o and N_i, or

$$E_{oi} = P_o + N_i.$$

For each simulated election the states can be ordered from most Republican to most Democratic, and the pivotal state observed.

To simulate a set of elections in a sector we first locate an election vector (a P_o) in the most counterclockwise orientation in the sector, and then systematically alter that orientation in two-degree units in a clockwise direction through the sector. For each orientation, 10,000 different random vectors (10,000 N_i) are constructed; each random vector provides the basis for a unique simulated election. This method assures that the distribution within sectors will be uniform and will include a full range of orientations. The exact number of simulations per sector using this procedure is 260,000 in the ideology sector, 210,000 in the mixed sector, 470,000 in the modern (ideology plus mixed) sector, and 240,000 in the party sector.

In sum, we are simulating many hypothetical elections in each of the sectors. Each simulated election entails two steps. First, we place in the space an axis on which the states are projected. Second, the position of each state is altered by a random perturbation which represents state-specific effects and reflects uncertainty about the exact state position in the ordering. The power of a state in each sector is determined by the percentage of times it occupies the critical pivot position.

Washington, D.C.

One final point needs to be considered before we present the power values: the case of the District of Columbia. We had inadequate data to include Washington, D.C., in the component analysis. We did, however, do so in preliminary analyses in which we estimated missing votes for the District. In all instances the District of Columbia was located far from the main cluster of states in the Democratic and liberal direction. Thus, in analyzing power we followed two strategies. The first was simply to ignore the District and assume that there were only 535 electoral votes; the second was to include the District automatically as part of the Democratic coalition. We feel the latter is the more realistic way to proceed. The power values were virtually indistinguishable in the two cases; the values we report are those obtained including the District automatically as part of the Democratic coalition.[8]

The Power of the States

The Modern Sector

The three most powerful states in the modern sector are California, Texas and New York. These are the states with the largest populations and the largest number of electoral votes. The simple order, however, does not reveal the extent to which competitiveness influences the power distribution. Table 4 shows state power in the modern sector. Column 1 is the power value, showing the percentage of times the state occupies the pivotal position. That percentage is converted to electoral vote units in Column 3 by multiplying the power value by the 538 electoral votes. This value represents the number of electoral votes the various states would have if the number of electoral votes were proportional to power. Column 2 shows the actual electoral votes of the state, and column 4 shows power in electoral vote units using the traditional Shapley value without any consideration of competitiveness. In column 5 is the difference between the power measures, which indicates the extent to which state competitiveness influences state power.

The potent position of California is striking. California's electoral vote of 47, which a traditional Shapley analysis would increase to 49.9, is enhanced by our measure to 64.6, equivalent to a gain of almost 15 electoral units over the Shapley value. Texas, the second on the list and significantly less powerful, is also quite advantaged by its spatial position as it picks up eight electoral units. In contrast, New York actually loses some strength—slightly more than one electoral unit. Of the 10 most powerful states in the modern sector, only New York and Florida are net losers, with Florida losing the equivalent of about three electoral votes.

The traditional view of a potent midwest is supported by the analysis. The two largest midwestern states, Illinois and Ohio, are the fourth and fifth most powerful states. The power of both states is enhanced by spatial position by more than six electoral vote units. The next 5 states among the 10 most powerful are Pennsylvania, Michigan, New Jersey, Florida, and North Carolina. Of these, the only one substantially advantaged by strategic (spatial) position is New Jersey, picking up more than three electoral units.

At the bottom of the list are the least powerful states. As one would expect, these states tend to be small and politically predictable. The one exception is Massachusetts which is not small, but is predictable. Massachusetts is far and away the biggest loser in voting strength, dropping the equivalent of 11 electoral vote units, which in proportional terms leaves Massachusetts with less than one-seventh of its original voting strength.

The Ideology, Mixed, and Party Sectors

The discussion to this point has focused exclusively on power in the modern sector. We initiated our discussion with the modern sector because we feel future elections are likely to fall somewhere within that sector. We shall now consider the question of state power within the more specific ideology, mixed, and party sectors. Appendix 3 shows power in electoral vote units in the three sectors, along with the difference between the estimated power in the sector and that based on the size-only Shapley value.

Some states are consistently powerful and substantially advantaged by their spatial position—most notably California, Texas, Illinois and Ohio. Several other states, while not as large as these four, are also consistently advantaged by

Table 4. Power in the Modern Sector

Rank	State	Power %[a]	Electoral Votes	Power Electoral Votes[b]	Shapley Electoral Votes	Power-Shapley[c]
1	California	12.02	47	64.64	49.91	+14.74
2	Texas	6.99	29	37.60	29.60	+ 8.00
3	New York	6.70	36	36.02	37.29	− 1.27
4	Illinois	5.71	24	30.74	24.25	+ 6.50
5	Ohio	5.47	23	29.42	23.19	+ 6.23
6	Pennsylvania	4.96	25	26.68	25.31	+ 1.37
7	Michigan	3.97	20	21.37	20.05	+ 1.33
8	New Jersey	3.66	16	19.68	15.92	+ 3.77
9	Florida	3.36	21	18.09	21.09	− 3.00
10	North Carolina	2.53	13	13.63	12.86	+ 0.77
11	Missouri	2.41	11	12.95	10.84	+ 2.11
12	Wisconsin	2.34	11	12.57	10.84	+ 1.73
13	Washington	2.33	10	12.52	9.84	+ 2.68
14	Tennessee	2.22	11	11.93	10.84	+ 1.08
15	Indiana	2.19	12	11.78	11.85	− 0.07
16	Maryland	2.16	10	11.62	9.84	+ 1.78
17	Kentucky	2.02	9	10.86	8.84	+ 2.02
18	Virginia	1.91	12	10.29	11.85	+ 1.56
19	Louisiana	1.85	10	9.95	9.84	− 0.11
20	Connecticut	1.60	8	8.62	7.84	+ 0.78
21	Iowa	1.56	8	8.37	7.84	+ 0.52
22	Oregon	1.53	7	8.22	6.85	+ 1.37
23	Colorado	1.48	8	7.93	7.84	+ 0.10
24	Georgia	1.47	12	7.89	11.85	− 3.96
25	Minnesota	1.32	10	7.10	9.84	− 2.74
26	South Carolina	1.26	8	6.78	7.84	− 1.06
27	Alabama	1.26	9	6.77	8.84	− 2.07
28	Arkansas	1.07	6	5.77	5.86	− 0.09
29	New Mexico	1.06	5	5.69	4.87	+ 0.82
30	Oklahoma	0.95	8	5.08	7.84	− 2.76
31	West Virginia	0.85	6	4.57	5.86	− 1.29
32	New Hampshire	0.83	4	4.45	3.89	+ 0.56
33	Montana	0.82	4	4.39	3.89	+ 0.50
34	Mississippi	0.78	7	4.17	6.85	− 2.68
35	Nevada	0.76	4	4.06	3.89	+ 0.17
36	Maine	0.73	4	3.91	3.89	+ 0.02
37	Delaware	0.65	3	3.51	2.91	+ 0.60
38	Kansas	0.63	7	3.41	6.85	− 3.44
39	Alaska	0.62	3	3.34	2.91	+ 0.43
40	Arizona	0.62	7	3.33	6.85	− 3.52
41	South Dakota	0.57	3	3.07	2.91	+ 0.16
42	Hawaii	0.52	4	2.77	3.89	− 1.12
43	Vermont	0.44	3	2.39	2.91	− 0.53
44	North Dakota	0.37	3	2.00	2.91	− 0.92
45	Massachusetts	0.33	13	1.78	12.86	−11.08
46	Utah	0.32	5	1.75	4.87	− 3.12
47	Wyoming	0.27	3	1.46	2.91	− 1.46
48	Nebraska	0.26	5	1.42	4.87	− 3.45
49	Idaho	0.19	4	1.02	3.89	− 2.87
50	Rhode Island	0.12	4	0.64	3.89	− 3.25

[a] Percentage of times the state occupies the pivotal position.

[b] Percentage of times the state occupies the pivotal position multiplied by the total number of electoral votes (538).

[c] Difference between power measures.

spatial position. These states are New Jersey, Missouri, Washington, Maryland, and Kentucky.

In contrast, some states show substantial variation in their power based on election type. New York shows an increase of almost 30% in its power between the ideology and mixed sectors. The southern states as a group account for almost 25% of the total power in the mixed sector, approximately 21% in the ideology sector, and approximately 18% in the party sector. Massachusetts and Rhode Island, both extremely weak in the ideological and mixed sectors, are mildly advantaged in the party sector. Overall, while there is considerable consistency in power across sectors, real change does occur for some states.

The Power of Individual Voters

> To the extent that a citizen's right to vote is debased, he is that much less a citizen. The fact that an individual lives here or there is not a legitimate reason for overweighting or diluting the efficacy of his vote. The complexions of societies and civilizations change, often with amazing rapidity. . . . But the basic principle of representative government remains, and must remain, unchanged—the weight of a citizen's vote cannot be made to depend on where he lives. (Warren, *Reynolds v. Sims*, 1964)

Since the 1962 decision in the case of *Baker v. Carr* the Supreme Court has unequivocally supported the one-man one-vote ideal. Yet, this principle is regularly violated in the contest for the presidency. In presidential elections some citizens, by virtue of their physical location in a given state, are in a far better position to determine presidential outcomes than others.

In his widely cited article, "One Man 3.312 Votes," Banzhaf (1968) was the first to argue mathematically that substantial differences exist between the power of the most and least advantaged citizens. His analysis is based on the Shapley value and the assumptions that (1) the election involves only two competing candidates, and (2) the probability of a voter selecting one or the other candidate in the pivotal state is exactly the same (0.5). Under these assumptions the power of voters in one state (state A) in comparison to voters in another state (state B) is equal to the ratio of the power of each of the states divided by the square root of population size. That is:

$$\frac{\text{power state A/}\atop\text{square root of population size state A}}{\text{power state B/}\atop\text{square root of population size state B}}$$

Based on the 1960 census and using this formula, Banzhaf found that the citizens of New York had more than three times the power of the citizens of Washington, D.C., to determine the outcome of presidential elections.

Based on the Shapley value, the Banzhaf formulation does not take account of the political competitiveness of states. Therefore, we replaced the Shapley value as the measure of state power with our measure of state power in the modern sector, retaining the Banzhaf assumptions. When the relative power differentials are computed in this way, the results are far more striking. We find that a citizen of California is more than twenty times (20.2) as powerful as a citizen of Rhode Island.

These results are not solely dependent on the California and Rhode Island cases. The extreme ratio, ignoring both California and Rhode Island, is 13.4, between the citizens of Texas and Massachusetts. Even if we compare the state with the tenth most powerful citizens (Missouri) and the state with the tenth least powerful citizens (Mississippi), the power ratio exceeds two. This demonstrates that the unequal power of citizens in the United States is not merely due to idiosyncratic cases, but is generic to the Electoral College system.[9] Indeed, citizens of Washington, D.C., who are the most powerdeprived, are not even included in this assessment.

The power differentials between citizens in the ideology, mixed, and party sectors are larger than in the combined modern sector. This makes sense, because as the election type becomes more structurally defined, the power differential between states should tend to increase.[10] The ratios between the least and most powerful citizens are 41.4 for the ideology sector, 40.2 for the mixed sector, and 91.3 for the party sector. The extreme powerlessness of Georgia generates the very high ratio in the party sector. Similar powerlessness occurs for the citizens of several of the southern states. The low turnout in the South in the era of party-oriented elections is quite consistent with, though not necessarily caused by, the power deprivation of southerners in those elections. In sum, the results from the specific sectors serve to reinforce that in U.S. presidential elections, extreme inequities exist between the power of citizens living in different states.[11]

Methodological Considerations

The criticism most likely to be directed against the power model we have constructed is that slight changes in the model could be associated with substantial changes in the power values. The questions we feel need to be addressed in this regard are the following:

(1) What would happen if we reestimated the structural model, recognizing that the original election percentages might be biased because of home state effects?
(2) How sensitive is the model to the decision to allocate half of the variance to previous structure and half to election-specific (random) factors?
(3) Would the results change if we used different elections to determine the structure?

In order to address these questions we repeated the entire power analysis, first correcting for possible biases due to the home state of the presidential candidates.[12] We then repeated the power analysis letting state position be a function of 60% prior structure and also 40% prior structure, rather than the 50% used in the analysis we have already reported. We did this for both the home state adjusted and original structures. Finally, we repeated the analysis using just the 1960–1980 elections to generate the spatial structure, and performed a power analysis on the basis of that structure. We performed the 1960–1980 analyses using both the unadjusted and home state corrected percentages.

The stability of the results is striking. Table 5 shows the correlations between the power estimates in the modern sector using each of these methods. No correlation is below 0.99. Shown in parentheses are the correlations between the variables formed by subtracting the state's Shapley value from the power value for each state. This subtraction removes any state size influence which would tend to increase the correlations. Every correlation among these size-modified values is above 0.95. These correlations indicate the method is exceedingly robust, and the power values recovered extremely stable.

Political Observations

Mexican Americans

A full analysis of the demographic implications of the power of the states is beyond the intended scope of this article. However, the role of Mexican Americans is so obviously relevant that some discussion is warranted.[13] Mexican Americans are becoming an increasing percentage of the U.S. population. As a group they display relatively low turnout; hence, if mobilized, they are capable of having a pronounced impact on elections. Mexican

Table 5. Correlations Between Power Values[a]

Model	Actual Measure 50% Random (Power 50)	40% Random (Power40)	60% Random (Power60)	1960–80 Data 50% Random (1960–80)	Home Adjusted 50% Random (Power 50A)	Home Adjusted 40% Random (Power 40A)	Home Adjusted 60% Random (Power 60A)	Home Adjusted 1960–80 Data 50% Random (1960–80A)
POWER50	1.000 (1.000)							
POWER40	0.998 (0.993)	1.000 (1.000)						
POWER60	0.998 (0.980)	0.992 (0.975)	1.000 (1.000)					
1960–80	0.998 (0.980)	0.995 (0.968)	0.997 (0.980)	1.000 (1.000)				
POWER50A	0.998 (0.980)	0.998 (0.974)	0.997 (0.977)	0.998 (0.981)	1.000 (1.000)			
POWER40A	0.994 (0.969)	0.995 (0.974)	0.990 (0.955)	0.994 (0.967)	0.997 (0.993)	1.000 (1.000)		
POWER60A	0.997 (0.977)	0.997 (0.960)	0.999 (0.984)	0.997 (0.982)	0.998 (0.994)	0.991 (0.977)	1.000 (1.000)	
1960–80A	0.998 (0.981)	0.995 (0.974)	0.997 (0.976)	0.997 (0.994)	0.999 (0.991)	0.996 (0.982)	0.998 (0.986)	1.000 (1.000)

[a] In parentheses are the correlations between size-modified power values.

Americans live predominanty in the southwest. According to the 1980 census they composed 19% of the population of California and 21% of the population of Texas, the two most powerful states. These percentages, even with no further immigration, are sure to increase over the next several decades, since Mexican Americans as a group are both younger and have a substantially higher fertility rate than the non-Mexican American population. Because Mexican Americans are likely to be especially influential in the two most powerful states, they are likely as a group to have a disproportionate impact in future close elections.

Are the Republicans Smarter?

We would expect candidates from powerful states to be advantaged in presidential elections for two reasons. First, the more powerful a state, the greater the strategic value of a home state boost. Second (and, we believe, more critical), large competitive states provide the political environment most comparable to presidential elections. Thus, it is likely that a political style successful in a powerful state will carry over nicely to a presidential election campaign. Candidates from powerful states are, of course, not guaranteed to run well. Other things being equal, however, we would expect them to be very competitive.

Over the 1944–1984 time frame the Republican Party has selected candidates from powerful states far more frequently than has the Democratic Party. Indeed, in every election in the period except for 1944 and 1964, the Republican Party has nominated a candidate from the stronger state, and in 1944 both were New Yorkers.[14] Likewise, in every election in the period except for 1944 and 1964 the Republican candidate has run ahead of the Republican congressional delegation. The Republican sensitivity to California has also been impressive; a Republican presidential or vice-presidential nominee has come from California in 8 out of the 11 elections in the period. The 1980 and 1984 Reagan-Bush tickets were optimal from a state power perspective, with the presidential candidate representing the most powerful state and the vice-presidential candidate the second most powerful.

Conclusion

The Electoral College is a uniquely American political institution. Because states are the units that choose the president in the Electoral College system, the relative power of the states becomes politically significant. We have developed a model of state power which takes advantage of the pivotal player notion critical in game theory analyses of power, but which at the same time recognizes that states differ in terms of competitiveness. The resulting analyses show California, Texas, Illinois, and Ohio to be substantially more important actors in modern presidential elections than one would expect, even taking into account their large size. Massachusetts and Rhode Island, in contrast, are substantially less powerful. Understanding disparities in state power is the necessary foundation for evaluating the power of demographic groups and presidential campaign strategies.[15] These will be relevant subjects for further analysis.

A critical function of empirical political science is to inform the broader society on issues of normative importance. The question of the inequality of voters in presidential elections is such an issue. Our analysis indicates that far greater differences in power exist across the voting population than previous research has shown. Using the common Banzhaf assumptions and taking competitiveness into account, we find a citizen of California is over 20 times more likely to determine the outcome of a modern presidential election than is a citizen of Rhode Island. Even eliminating California and Rhode Island, we find ratios above 13 between the power of the most and least advantaged citizens. Whether these departures from the one-man one-vote principle are viewed with alarm is, of course, a normative decision. That substantial disparities arise because of the Electoral College system is now apparent.

Appendix 1. Model and Data

If (1) the voting choices of individuals conform to a multidimensional spatial model, and (2) the distribution of voters in each of the 50 states is multivariate normal, with states having different mean locations but identical covariance structures, then principal components provide a least squares estimate of the spatial position of state means and the orientation of election vectors.

Notation

We will use upper-case letters to represent vectors and matrices, and lower case letters to represent scalars. Column vectors will be denoted with a $'$; thus X is a matrix or a row vector, X' is the transpose of X, and x is a scalar.

Proof

Let θ_{re} represent the spatial location of the Republican candidate in election e, and θ_{de} the location of the Democratic candidate. Let $\Delta_e = \theta_{re} - \theta_{de}$. (Note: Δ_e is the election vector.) The equation of the bisecting hyperplane can be represented as

$$\Delta_e \cdot X' = k_e$$

where k_e is an election specific constant. (See Thomas, 1963, pp. 620–21 for a discussion of the geometry.) Hence if V_i' is the spatial position of voter i

$\Delta_e \cdot V_i' > k_e$ implies voter i will vote Republican.

$\Delta_e \cdot V_i' < k_e$ implies voter i will vote Democratic.

Let $\overline{V}_s$ be the mean location of the voters in state s. Now consider the distribution of the $\Delta_e V_i'$ in some state s. This distribution is univariate normal with the mean $\Delta_e \overline{V}_s'$. This follows from the fact that linear combinations of normal random variables are normally distributed. The variance of each of the state distributions would be identical across states because they have identical covariance structures; call that variance σ_e^2.

The proportion of the voters in state s in election e who vote Democratic would thus be

$Pr[-\infty < z < (k_e - \Delta_e \overline{V}_s')/\sigma_e]$
where z is a standard normal variate.

Let $P_{s,e} = k_e/\sigma_e - (1/\sigma_e)\ \Delta_e \dot{\overline{V}}_s'$. [Note: $P_{s,e}$ is the probit value associated with the above probability range.]

Now consider the 50 element vector P_e composed of the various $P_{s,e}$. $P_e = K^*_e - \Delta^*_e \overline{V}'$ where K^*_e is a vector of constant k_e/σ_e, $\overline{V}'$ is the matrix formed with each of the $\overline{V}_s'$ as columns, and Δ^*_e is $(1/\sigma_e)\ \Delta_e$.

We have not established an origin for the structural space. Let the mean of each row of $\overline{V}'$ be zero, which puts the origin of the political space in the center of state means. The average value of the elements of the vector formed by $\Delta^*_e \overline{V}'$ would then be zero for any Δ^*_e.

Let Z_e be the standardized version of P_e, that is $Z_e = -(1/\sigma\text{-}_e)\ (P_e - K^*_e)$ where σ_{p_e} is the standard deviation of the values of P_e.

$Z_e = -(1/\sigma_{p_e})\ \Delta^*_e \overline{V}'$ or

$Z_e = \Delta^{\#}_e V'$ where $\Delta^{\#}_e = -(1/\sigma_{p_e})\Delta^*_e$.

Now consider the matrix Z whose rows are the various Z_e and the matrix $\Delta^{\#}$ whose rows are the various $\Delta^{\#}_e$. We then have:

$Z = \Delta^{\#}\overline{V}'$.

Notice this model is specified only up to a unit of measurement on each row of $\overline{V}'$. Let the variance of the value of each row of $\overline{V}'$ be equal to one. The model is now the classic component model and principal components provide a least squares estimate of $\Delta^{\#}$ and $\overline{V}'$.

Data

The scaling model depends on the Δ_e vectors above being well defined, which implies that only two candidates, the Democratic and the Republican, are competing. Hence, in elections with a significant minor party vote the results need to be modified suitably. We used the following method to modify the percentages:

(a) In 1968 we divided each Wallace vote .685 for Nixon to .315 for Humphrey. This division was based on the thermometer preference ratio of Nixon to Humphrey among those respondents who indicated they had voted for Wallace in the 1968 national survey of the Inter-University Consortium for Political and Social Research (ICPSR). (The .685 assigned to Nixon is the sum of the percent preferring Nixon to Humphrey, plus one-half of the ties.) In 1980 an identical method was used to distribute the Anderson vote in the ratio of .555 for Carter to .445 for Reagan. We rely on thermometer ratings to distribute the Wallace and Anderson votes, since they are by far the best single predictor of vote available in the ICPSR national survey.

(b) In states other than Alaska and Hawaii, in which more than 7.5% of the total vote was for other than the two major party candidates (excluding Wallace in 1968 and Anderson in 1980), we calculated a regression estimate of the Democratic percent of the vote. The procedure involved first regressing the election outcomes for each of the years (other than 1952 and 1968) on the 1952 and revised 1968 Democratic percentages. These two elections had the desired characteristics of having no missing data and being quite different kinds of elections. We then used the regression coefficients to calculate the estimated vote, making sure each estimate was plausible in that the estimated Democratic percent had to be greater than the actual Democratic vote, and less than 100.0—the Republican percent. If the estimate was less than the actual Democratic percent, the actual percent was used. If the estimate was more than 100—the Republican percent, 100—the Republican percent was used. Estimates were used in the following cases and are reported as (original to estimated):

1948: Alabama (0 to 58), Louisiana (33 to 54), Mississippi (10 to 57), New York (45 to 51), South Carolina (24 to 53), Tennessee (49 to 52), Virginia (48 to 50);
1956: South Carolina (45 to 47);
1960: Louisiana (50 to 52), Mississippi (36 to 55);
1972: Idaho (26 to 30).

(c) For Alaska and Hawaii all votes prior to 1960 were estimated, based on regressions using 1960 and 1968 as the independent variables, as was Alaska's vote in 1980. The estimates for Alaska and Hawaii by year are: 1944: (55, 47); 1948: (50, 50); 1952: (44, 39); 1956: (42, 37); 1980: Alaska (26 to 35). Since a substantial number of Alaska and Hawaii's vote percentages are estimates, their spatial placements should be treated with caution.

(d) The source for the 1944-1972 presidential election data was ICPSR study #0019 *State Level Presidential Election Returns* (1824-1972), which was originally collected by Walter Dean Burnham, Jerome M. Clubb, and William H. Flanigan. (We corrected one keypunch error where the 1968 Democratic and Republican vote in Oregon had been reversed.) The 1976 and 1980 official returns were obtained from *Congressional Quarterly Weekly Reports* (1976: December 18, 1976, pp. 3334-3335; 1980: January 17, 1981, p. 138).

(e) In the analysis we used actual percentages rather than probit values. Probit values are virtually linear with percentages in the 20-80% range. The few cases in which the Democratic percentage fell below 20% were associated with ballot irregularities. These artifactually low percentages are only exaggerated by a probit transformation; hence we felt such a transformation was inappropriate.

Appendix 2. Predicted and Actual State Orders from Most Republican to Most Democratic

1952 Party		1972 Ideology		1980 Mixed	
Predicted	Actual	Predicted	Actual	Predicted	Actual
Vermont	Vermont	Mississippi	Mississippi	Nebraska	Utah
Nebraska	North Dakota	Alabama	Oklahoma	Idaho	Idaho
Kansas	Kansas	Georgia	Georgia	Kansas	Nebraska
North Dakota	South Dakota	South Carolina	Alabama	North Dakota	North Dakota
Idaho	Nebraska	Utah	Utah	Wyoming	Nevada
South Dakota	Maine	Oklahoma	South Carolina	Utah	Wyoming
Wyoming	Idaho	Louisiana	Florida	Vermont	Arizona
Maine	Iowa	Arizona	Louisiana	Arizona	South Dakota
Iowa	Wyoming	Idaho	North Carolina	Oklahoma	New Hampshire
Colorado	Nevada	Florida	Nebraska	Colorado	Alaska
Wisconsin	Wisconsin	North Carolina	Kansas	South Dakota	Oklahoma
Hawaii	Oregon	Nebraska	Tennessee	Iowa	Montana
New Hampshire	Colorado	Tennessee	Arizona	Indiana	Kansas
Utah	New Hampshire	Arkansas	Idaho	Virginia	Colorado
Nevada	Hawaii	Virginia	Virginia	Nevada	New Mexico
Indiana	Montana	Wyoming	Wyoming	New Hampshire	Indiana
Oregon	Indiana	Kansas	Arkansas	Montana	California
Montana	Utah	Indiana	Texas	Alaska	Florida
Arizona	Arizona	Texas	Indiana	Florida	Texas
New Jersey	New Jersey	New Mexico	Colorado	New Mexico	New Jersey
Alaska	California	Nevada	Alaska	Wisconsin	Virginia
Connecticut	Ohio	Kentucky	Kentucky	Oregon	Washington
Maryland	Virginia	Alaska	New Hampshire	New Jersey	Iowa
New Mexico	New York	North Dakota	North Dakota	Maine	Oregon
New York	Maryland	Montana	Nevada	South Carolina	Ohio
Virginia	Connecticut	Colorado	West Virginia	Tennessee	Connecticut
Ohio	*Michigan*	New Hampshire	New Mexico	Ohio	*Illinois*
Michigan*	Minnesota	Missouri*	Vermont	Texas	Pennsylvania
Illinois	Alaska	Delaware	New Jersey	Mississippi	Michigan
California[a]	New Mexico	*California*	Maryland	*California*	Missouri
Oklahoma	Washington	Washington	Hawaii	Illinois*	Vermont
Washington	Illinois	Iowa	*Missouri*	Maryland	Louisiana
Minnesota	Florida	Ohio	Montana	North Carolina	Wisconsin
Massachusetts	Oklahoma	Illinois	Ohio	Louisiana	Maine
Texas	Massachusetts	New Jersey	Maine	Washington	Alabama
Florida	Texas	South Dakota	Washington	Connecticut	New York
Pennsylvania	Pennsylvania	Oregon	Pennsylvania	Kentucky	Delaware
Rhode Island	Delaware	Pennsylvania	Delaware	Michigan	North Carolina
Missouri	Rhode Island	Maryland	Connecticut	Missouri	Mississippi
Delaware	Missouri	West Virginia	Iowa	Delaware	Kentucky
Kentucky	Tennessee	Vermont	Illinois	New York	Arkansas
Tennessee	Kentucky	Wisconsin	New York	Hawaii	South Carolina
West Virginia	South Carolina	Michigan	California	Pennsylvania	Tennessee
North Carolina	West Virginia	Connecticut	Michigan	Minnesota	Massachusetts
Louisiana	Louisiana	New York	Oregon	Alabama	Hawaii
South Carolina	North Carolina	Minnesota	Wisconsin	Arkansas	Minnesota
Arkansas	Arkansas	Maine	South Dakota	West Virginia	Maryland
Mississippi	Mississippi	Hawaii	Minnesota	Georgia	West Virginia
Alabama	Alabama	Rhode Island	Rhode Island	Rhode Island	Rhode Island
Georgia	Georgia	Massachusetts	Massachusetts	Massachusetts	Georgia

*Denotes state occupying the actual pivot position.

[a] Italics denote state occupying the pivot positions in the column.

Appendix 3. State Power in Electoral Vote Units by Sector

	Ideology			Mixed			Party		
Rank	State	Power	Difference from Shapley	State	Power	Difference from Shapley	State	Power	Difference from Shapley
1	California	66.36	+16.46	California	62.52	+12.61	California	61.32	+11.41
2	Texas	37.79	+ 8.18	New York	41.10	+ 3.81	New York	44.11	+ 6.83
3	New York	31.92	− 5.37	Texas	37.37	+ 7.77	Texas	35.35	+ 5.75
4	Illinois	31.34	+ 7.09	Illinois	30.01	+ 5.76	Pennsylvania	29.35	+ 4.04
5	Ohio	30.09	+ 6.90	Ohio	27.59	+ 5.40	Illinois	28.89	+ 4.64
6	Pennsylvania	27.59	+ 2.28	Pennsylvania	25.55	+ 0.24	Ohio	27.53	+ 4.34
7	Michigan	20.21	+ 0.17	Michigan	22.81	+ 2.76	Michigan	23.98	+ 3.93
8	New Jersey	20.00	+ 4.08	Florida	21.51	+ 0.42	Florida	23.87	+ 2.77
9	Florida	15.32	− 5.77	New Jersey	19.29	+ 3.37	New Jersey	17.62	+ 1.70
10	Missouri	13.42	+ 2.58	North Carolina	15.41	+ 2.56	Virginia	13.75	+ 1.90
11	Indiana	13.03	+ 1.19	Tennessee	13.11	+ 2.27	Massachusetts	13.18	+ 0.32
12	Washington	12.82	+ 2.98	Wisconsin	12.66	+ 1.81	Indiana	12.19	+ 0.34
13	Wisconsin	12.50	+ 1.66	Missouri	12.37	+ 1.53	Missouri	12.10	+ 1.26
14	North Carolina	12.19	− 0.67	Maryland	12.23	+ 2.39	Washington	11.91	+ 2.08
15	Kentucky	11.42	+ 2.58	Washington	12.14	+ 2.31	Maryland	11.66	+ 1.83
16	Maryland	11.13	+ 1.30	Louisiana	11.75	+ 1.92	Minnesota	11.45	+ 1.61
17	Tennessee	10.97	+ 0.13	Virginia	10.26	− 1.59	Tennessee	10.87	+ 0.03
18	Virginia	10.32	− 1.53	Indiana	10.22	− 1.63	North Carolina	10.35	− 2.51
19	Georgia	10.14	− 1.71	Kentucky	10.17	+ 1.33	Kentucky	9.91	+ 1.07
20	Iowa	9.82	+ 1.98	Connecticut	9.32	+ 1.48	Wisconsin	9.83	− 1.01
21	Colorado	9.43	+ 1.59	South Carolina	8.96	+ 1.11	Connecticut	8.90	+ 1.06
22	Louisiana	8.48	− 1.35	Minnesota	8.33	− 1.51	Oklahoma	8.81	+ 0.97
23	Oregon	8.45	+ 1.60	Oregon	7.93	+ 1.08	Oregon	7.33	+ 0.48
24	Connecticut	8.05	+ 0.21	Alabama	7.68	− 1.16	Louisiana	7.04	− 2.80
25	Arkansas	6.69	+ 0.83	Mississippi	7.13	+ 0.28	Arizona	6.91	+ 0.06
26	Minnesota	6.11	− 3.73	Iowa	6.56	− 1.28	Colorado	6.73	− 1.12
27	Alabama	6.04	− 2.80	Colorado	6.08	− 1.76	New Mexico	5.85	+ 0.97
28	New Mexico	5.96	+ 1.08	Oklahoma	5.82	− 2.02	Iowa	5.80	− 2.04
29	Kansas	5.29	− 1.56	New Mexico	5.36	+ 0.49	South Carolina	5.57	− 2.27
30	West Virginia	5.18	− 0.68	Georgia	5.10	− 6.74	West Virginia	5.21	− 0.65
31	South Carolina	5.02	− 2.82	Arkansas	4.63	− 1.23	Utah	4.65	− 0.22
32	New Hampshire	4.98	+ 1.09	Maine	4.51	+ 0.62	Montana	4.19	+ 0.30
33	Montana	4.85	+ 0.95	Hawaii	4.01	+ 0.12	Rhode Island	3.98	+ 0.09
34	Nevada	4.52	+ 0.63	Montana	3.82	− 0.07	Nevada	3.97	+ 0.08
35	Oklahoma	4.49	− 3.35	West Virginia	3.81	− 2.05	New Hampshire	3.81	− 0.08
36	Delaware	3.78	+ 0.86	New Hampshire	3.79	− 0.10	Hawaii	3.78	− 0.11
37	South Dakota	3.54	+ 0.63	Nevada	3.49	− 0.40	Alaska	3.39	+ 0.47
38	Alaska	3.53	+ 0.61	Arizona	3.18	− 3.67	Delaware	3.21	+ 0.29
39	Arizona	3.45	− 3.40	Delaware	3.17	+ 0.26	Maine	2.82	− 1.07
40	Maine	3.43	− 0.47	Alaska	3.11	+ 0.19	Kansas	2.38	− 4.47
41	Vermont	3.30	+ 0.39	Massachusetts	3.02	− 9.84	Arkansas	2.09	− 3.77
42	North Dakota	2.87	− 0.05	South Dakota	2.50	− 0.42	Wyoming	2.01	− 0.91
43	Nebraska	2.25	− 2.63	Utah	1.66	− 3.21	Mississippi	1.92	− 4.93
44	Wyoming	1.90	− 1.02	Vermont	1.25	− 1.66	Idaho	1.90	− 2.00
45	Utah	1.82	− 3.06	Kansas	1.08	− 5.77	South Dakota	1.82	− 1.09
46	Mississippi	1.78	− 5.07	Rhode Island	0.99	− 2.91	Nebraska	1.65	− 3.22
47	Hawaii	1.77	− 2.12	North Dakota	0.92	− 2.00	North Dakota	1.09	− 1.82
48	Idaho	1.51	− 2.38	Wyoming	0.91	− 2.00	Alabama	1.09	− 7.75
49	Massachusetts	0.79	−12.07	Idaho	0.42	− 3.48	Vermont	0.59	− 2.33
50	Rhode Island	0.36	− 3.53	Nebraska	0.40	− 4.43	Georgia	0.32	−11.53

Notes

We would like to thank Jeffrey Obler and the Comparative Politics Discussion Group of the University of North Carolina for useful comments on an earlier version of this manuscript.

1. The two attempts that have been made to assess state power more systematically, Merrill (1978) and Levesque (1984), both have substantial drawbacks. Merrill's analysis suffers from a naive view of the electoral process. Levesque's analysis is based on a probability model which is seriously flawed and which yields assessments lacking even surface validity. Levesque assesses the probability that a state, by voting one way or another, might sway an Electoral College outcome. In calculating this probability he does not consider the likelihood that the state will actually be close to the 50-50 split. Indeed, he gives no consideration whatever to the probable behavior of the key state. The measure is akin to assessing the likely hero in a football game by seeing how evenly balanced the two teams are given a certain player is excluded, but ignoring the quality of that player. Thus, it is not surprising that Levesque's results are counter-intuitive. For example, in 1952 (Levesque only provides estimates for 1952 and 1968) he finds Georgia, Alabama, Mississippi, and North Carolina more powerful per electoral vote than either Illinois or Ohio, even though he calculates the probability of each of the four southern states voting Republican to be zero to four decimal places.

2. The spatial (structural) approach we take in analyzing state power is similar to that used by Shapley (1977) in a theoretical discussion of legislative behavior. In constructing our empirical model of state power we developed this approach independent of Shapley. We are indebted to one of the anonymous reviewers for alerting us to the Shapley paper.

3. This result is consistent with individual level structural (spatial) analyses of the U.S. electorate. See, for example, Weisberg and Rusk (1970), Rabinowitz (1978), Enelow and Hinich (1984), and Poole and Rosenthal (1984).

4. More generally, principal component based methodologies are proving useful for spatial analysis. In particular see Enelow and Hinich (1984) and Aldrich and McKelvey (1977).

5. The exact method used to construct the party variable was to average the Republican David Composite B index for each state over all of the off year elections from 1942–1978. The Composite B is itself an average of Senate, House, and Gubernatorial election data. The liberal-conservative variable was based on the difference between the Americans for Democratic Action (ADA) and Americans for Constitutional Action (ACA) rating of each state congressional delegation in each year such ratings were available (1960–1980). The ADA-ACA difference was calculated for each member of the state delegation, and these differences were averaged over the entire time span to form the single state liberal-conservative value. The Composite B and liberal-conservative variables were then used as dependent variables in regressions where the spatial dimensions were the independent variables. The axes drawn in the space are those in which the state projections most closely (in a least squares sense) approximate the original two variables. The R^2 for the party regression was .793, while that for the liberal-conservative regression was .741. Both of these values indicate very good fit.

6. Asher (1984) provides a good summary of the literature on presidential elections from 1952 through 1980. The party orientation of the early elections corresponds exactly to standard survey findings for those elections. The three elections in which the ideological component was predominant are all elections in which issues played a substantial role. That an ideological dimension can summarize issue effects well, even when mass publics tend not to be ideological, makes sense if elites behave in an ideologically consistent fashion. This idea is discussed at greater length in Rabinowitz, Gurian, and Macdonald (1984).

7. We state the boundary margins with such precision because we will estimate state power within sectors; hence, the sectors must be well defined. Our particular choice of these boundaries rests on the presumption that the 1964 and 1976 election axes set realistic bounds for any election in the near future. The reason that boundary changes have little impact is that the projections are quite stable for proximal axes. For the same reason, the method is not sensitive to the distribution of axes within sectors.

8. Including Washington, D.C., there are an even number of electoral votes (538). This means that ties are possible. In the event of ties, the state which brings the Democratic side to 269 and the state which brings the Republican side to 269 are both counted 0.5. By assigning 0.5 to each side in tied cases and 1 to the pivot state in untied cases, we are entirely consistent with the Shapley method. The Shapley method would count 2 for each untied case (the permutation and its mirror image) and 1 for each side in tied cases.

9. Recently Margolis (1977, 1983) has criticized Banzhaf's equal probability assumption, and shown that if the actual probabilities deviate only slightly from 0.5, the divisor (square root of population size) may be inappropriate. The implication of Margolis' critique is that more modest power differences may exist between individual voters than those computed by Banzhaf. While it is not within the scope of this paper to treat the question fully, we did compute a more conservative measure of the relative power of individuals. In order to be as conservative as possi-

ble, and in keeping with the Margolis critique, we found the divisor between the population size and the square root of population size which minimizes the relative advantage of any one citizen in comparison to any other citizen.

To obtain the minimizing divisor, we considered sets of divisors of the class:

> divisor = $a \cdot$ population + $(1-a) \cdot$ square root of population, $[0 < a < 1]$.

In particular we considered the 101 sets of divisors associated with "a" values from 0 to 1 separated by .01 (i.e., $a = 0, .01, .02, .03, \ldots 1.0$). We calculated the maximum power ratio for each set of divisors, and found the set which had the smallest maximum power ratio; this occurs when $a = .51$. Note that when $a = 1$ the set of divisors is the population size of each state, while when $a = 0$ the set of divisors is the square root of state population. The maximum power ratio is a nicely behaved function of "a" monotonically decreasing until its minimum, and monotonically increasing thereafter.

Using this minimizing divisor, we find the power of the citizens of California, who are the most powerful, to be more than eleven times (11.6) that of the citizens of Massachusetts, who are the least powerful. Setting aside these two states the ratio is 9.1. Clearly, these ratios are lower than those obtained with the Banzhaf assumptions. However, even using this very conservative approach, when the competitiveness of the states is taken into account, the differential power of citizens is substantial.

10. This follows directly from the fact that as the election axis changes orientations, states that are quite far from the center in one orientation will often tend to be closer to the center in another orientation.

11. Using the minimizing divisor (see footnote 9 above) the ratios between the least and the most powerful citizens are 27.2 for the ideology sector, 19.0 for the mixed sector, and 60.0 for the party sector.

12. Our bias correction entailed the following procedure. We regressed each election outcome on the ADA/ACA ideology variable and the David Composite B party variable. We then calculated the residual from the regression estimate for each state with a home state presidential candidate, except for 1944 and the Eisenhower cases. We eliminated 1944 because both candidates were New Yorkers; we eliminated the Eisenhower cases because he had no electoral history in any state.

The calculated residuals are reported below. Positive numbers indicate that the home state candidate ran better than the regression predicted, while negative numbers indicate the candidate ran worse. The residuals are: 1948: Missouri +6.45, New York +0.58; 1952: Illinois +4.71; 1956: Illinois +0.45; 1960: Mass +6.69, Calif −1.08; 1964: Texas +7.73, Arizona +2.94; 1968: Minn +6.22, Calif +0.95; 1972: South Dakota +10.31, Calif −3.19; 1976: Georgia +8.66, Michigan +1.55; 1980: Georgia +6.86, Calif +2.38.

Only Nixon in 1960 and 1972 ran worse in his home state than predicted by the regressions. We adjusted the actual vote in each state by the amount of the regression residual less 0.305, except in the 1960 and 1972 Nixon cases (0.305 is the sum of the two Nixon residuals divided by 14). In the ensuing home state adjusted factor analysis, no state with a candidate running had a residual in excess of 3.0 percent, except for Nixon in 1972 (4.1 percent).

We may have slightly overcorrected the home state influence using this method. The mean residuals from the home state adjusted factor analysis were slightly negative for both parties (i.e., home state adjusted candidates were predicted to do better than their adjusted percentages). For the purposes of checking the robustness of the power measure, overcorrection is not a problem, since the overcorrection increases the difference between the unadjusted and adjusted percentages.

The corrections we made are generally consistent with those implied by Lewis-Beck and Rice (1983). They use quite a different approach in assessing home state effects over the last century.

13. See Sigman (1983) for an excellent discussion of the potential impact of Mexican Americans on U.S. electoral politics.

14. The statement is objectively true for Eisenhower—he was formally a New Yorker in 1952 and a Pennsylvanian in 1956, both party-oriented elections. Eisenhower, however, was clearly not a candidate with strong state ties.

15. That the inadequacy of game theory treatments has hampered the development of theories of campaign strategies is well recognized. See, for example, the Colantoni, Levesque and Ordeshook (1975) critique of the important Brams and Davis (1974) piece.

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LEGISLATORS AND INTEREST GROUPS: HOW UNORGANIZED INTERESTS GET REPRESENTED

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This paper derives a supply price for public policy using a constrained maximization model. In the model, three sets of agents each have preferences over outcomes: organized interest groups offer campaign contributions to improve their own wealth, voters offer votes to obtain outcomes closer to their most preferred outcomes, and legislators seek both campaign contributions and votes to obtain reelection. A given legislator's supply price for policy is shown to depend on the productivity of his effort, as determined by committee assignments, priority and ability, and by the preferences of his unorganized constituency in the home district. Two extreme assumptions about the effectiveness of campaign spending in eliciting votes are used to illustrate the comparative statics properties of the model. The prediction of the model is that interest groups will, in general, seek out legislators whose voters are indifferent to the policy the interest group seeks. Thus, voters who do have preferences over policy are in effect represented, even though they are not organized.

The extent to which organized interest groups can affect policy is a matter of theoretical debate. Early pluralist theory in political science asserted that competition between groups is the sole process by which policy was formed, with the Congress (in the U.S.) as the passive referee in this struggle (see Bailey, 1950; Bentley, 1907; Latham, 1952; Schattschneider, 1935; and Truman, 1958, 1959; Niskanen, 1971, takes this view in discussing bureaucracy). "The legislative vote on any issue tends to represent the composition of strength, i.e., the balance of power among the contending groups at the moment of voting" (Latham, 1952, p. 36).

However, this view is clearly too simplistic. Congressmen are not ciphers, meekly responding to pressure-group activities. Partly as a reaction to the pluralist position, a dissenting school developed, marked by Matthews (1960), Bauer, Pool, and Dexter (1972), and

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

others. Bauer et al. (1972, p. 478) assert that

> congressmen have a great deal more freedom than is ordinarily attributed to them. The complexities of procedure, the chances of obfuscation, the limited attention constituents pay to any one issue, and the presence of countervailing forces all leave the congressman relatively free on most issues. He may feel unfree because of the great demands on his time, but, consciously or unconsciously, by his own decisions on what he chooses to make of his job he generates the pressures which impinge upon him. He hears from voters about those things in which he himself chooses to become involved.

The view of Congressional dominance did not completely replace the pluralist theory as the orthodox model of policy formation, but it was widely accepted. The new approach was of great importance because of the explicit recognition that policy formation is affected both by those who seek a certain outcome, as the pluralists claimed, and by those legislators who have discretion over who receives the benefits. More recent work views Bauer et al. as having overemphasized "supply" forces, giving Congress nearly complete discretion over policy and ignoring interest groups as demanders of policy. Efforts to explore the middle ground, where lobbyists and congressmen both share and compete with each other for decision power (Hayes, 1981, p. 12) have spawned case studies and theoretical analyses involving "subgovernments," "policy subsystems," "iron triangles," and "clientelism," but offer few attempts to model these relationships formally, and provide few general conclusions.

One explicit model of the supply and demand components of the process is Miller and Moe's (1983) bilateral monpoly model. While conceptually appealing, the bilateral monopoly model has few predictions for outcomes beyond the fact that the greater a group's bargaining strength, the better able it is to improve its strength against the others. However, many political situations have competitive aspects that make a bilateral monopoly model inappropriate. There are a number of national, or at least multidistrict, interest groups that may compete with other groups, or have the option of lobbying among several legislators.

Economists have generally used a different approach in attempting to discover, explain, and measure the effects that interest groups have in influencing legislators and legislation. For example, Stigler (1971), McPherson (1972), Pincus (1975), and Salamon and Seigfried (1977) all attempt to measure the effect of interest group power in obtaining favorable treatment. The same kind of technique has been used by Kau and Rubin (1982) and Peltzman (1982), using district-level aggregate data to measure the effect of demographic and other regularities of the constituency on roll-call voting behavior. While providing useful insights into the relationship between interest groups and congressmen, these studies are limited by the inability of survey or demographic data to measure accurately lobbying activity, constituent interests, and the preferences of legislators. Rather than explicitly modeling these disparate but important influences, the economic studies have simply identified regularities in the characteristics of interest groups and legislative voting behavior or policy outcomes.

This paper develops a theoretical model of the supply of public policy in the U.S. The model is shown to yield testable implications about which interest groups and which sets of voters are likely to be served. The basic logic of the supply model is as follows: Policy outcomes depend on the comparative advantages of the participants. Comparative advantage, in this context, depends on the value of what each set of agents in the model has to offer the others. We consider three sets of agents: voters, interest groups, and legislators. In order to simplify the analysis we focus on the implicit market for policy, in which interest groups offer contributions to legislators in exchange for legislators' efforts on each interest group's behalf. The contribution of this research is its derivation of an explicit *supply price* for policy. The amounts interest groups must offer a legislator for his services are shown to depend on that legislator's productivity and the preferences of the voters in his district.

Two extreme assumptions about voter preferences, which we distinguish by the names *rational ignorance* and *civics class*, are used to illustrate the comparative statics predictions of the model. The comparison of the results of these assumptions allows us to focus on the question of which factors influence the determination of groups to be served. Clearly, a variety of other comparative statics results are implied by the model, and these are discussed briefly in the conclusions.

The paper is divided into five parts. The first is a discussion of assumptions and definitions, followed by a version of the model that accounts for an incumbent legislator's choice of constituency. Section three examines the decision of interest groups to allocate their scarce resources among legislators. The fourth section examines the implications of the rationally ignorant and civics class assumptions about voter behavior for public policy outcomes. The last section is devoted to a summary and a discussion of extensions.

The Agents

Legislators

Legislators are assumed to maximize the number of votes they receive in the next election. While not literally accurate, maximizing votes is a sensible proxy, because a large margin in any given election affects the margin in the following election. That is, even if the legislator cares only about winning in each of the next several terms, he still acts as if he were maximizing votes in any contest except the last. As Mayhew (1974, p. 46) said, the goal of the elected official is to

> stay in office over a number of future elections, which does mean that 'winning comfortably' in any one of them . . . is more desirable than winning by a narrow plurality. The logic here is that a narrow victory (in primary or general election) is a sign of weakness that can inspire hostile political actors to deploy resources more intensively the next time around. By this reasoning, the higher the election percentage, the better.

Thus, maximization of vote received is a concise way to capture the motivation of a candidate involved in a campaign in which he is unsure of the identity or even of the probable characteristics of his opponent(s). Under these circumstances, strategic choice of activities reduces to a game against nature: the legislator actively searches for new ways to serve constituencies, impelled not by an actual opponent, but by the knowledge that he will face an attractive, well-financed challenger if he does not behave in this way.[1]

In order to influence votes received, legislators can provide services to any of *n* organized interest groups, or provide constituency services to the voters in their geographic district, by allocating a limited amount of effort to producing these services. This taxonomy was chosen over that of Lowi (1964) and others because their approaches involve a demand-oriented categorization of types of policy. In order to model more closely the sources of the services, we chose to use types of effort. Services to interest groups may include actual legislation, private bills, influence on the way bills are written (or vetoed) at the committee level, and the assurance that the "right" sources will be called on to testify at important hearings. Constituency services include casework, dealing with bureaucrats on a voter's behalf, and other individual services (Loewenberg and Patterson, 1979, pp. 187–90).

Voters

Votes come from voters, a heterogenous, unorganized group. They respond to the activities of the legislator, both in the legislature and in the district. In addition, because voters are assumed to be largely uninformed, resources devoted to advertising affect their responses. The type of advertising must be recognized as important. We acknowledge the legitimacy of Fiorina and Noll's (1979) criticism: "A major current area of inquiry in the legislative subfield is to explain the apparently overwhelming advantage of incumbency. Most . . . explanations seem to conflict with the basic spirit of rational choice majority rule models, in that citizens are implicitly assumed to respond to advertising regardless of content." It is our view that candidates do not accept this Galbraithian perspective, but rather have powerful incentives to control carefully the content of their advertising activities, both for unorganized groups and interest groups.

Interest Groups

Each interest group is assumed to be interested in only one policy. It offers a legislator campaign resources, in exchange for the expectation of future services. The definition of *resources* to be used here is Dahl's (1961, p. 226): "[A] resource is anything that can be used to sway the specific choices or the strategies of another individual." So resources include money, in-kind services, volunteer labor, etc.

Each of these three agents is assumed to be a constrained maximizer: Legislators maximize votes, subject to the constraint of total effort they can allocate. Voters' utility maximization, for our purposes, is completely embodied in the way they cast their one vote. Interest groups allocate scarce resources in the way that best furthers their goals.[2] All are constrained by the rules of the legislative institution, which is assumed to be a unicameral, majority-rule congress, with members elected from geographic districts.[3]

The Single Legislator Model

Consider an individual legislator who chooses activities that maximize the number of votes he expects to receive in the next election. Suppose there are n organized single interest groups, each of which can provide the legislator with campaign resources. That is, each group, i, can provide campaign resources, R_i, that the legislator will use in his next election campaign. In addition, there is a large unorganized group, U, each member of which has a vote to cast; aggregating these votes gives us the total voting resources, V_U, of the unorganized group. The unorganized group is assumed not to contribute campaign resources. The legislator then can choose from $n+1$ activities or types of policy services: constituency services—P_U—to group U, or policy services—P_1 through P_n—to the n single interest groups.

In order to decide which groups to serve, the legislator considers the total votes he will receive for each possible strategy. That is, he calculates whether he will devote effort, E_i, to produce each of the categories of service, P_i, where $i = 1$ to n. In making these calculations, he considers the response of each group to his activities:

for single-interest groups:

$$V_i = 0, i = 1 \text{ to } n \tag{1}$$

$$R_i = R_i[P_i(E_i)], i = 1 \text{ to } n; \tag{2}$$

total campaign resources:

$$R = R_1 + R_2 + \ldots + R_n; \tag{3}$$

for the unorganized group:

$$R_U = 0 \tag{4}$$

$$V = V[P_U(E_U), P_1(E_1), P_2(E_2), \ldots P_n(E_n), R]. \tag{5}$$

The explanation of the first four functions is straightforward. Equation (1) reflects the assumption that interest groups are nonvoting.[4] The second expression means that campaign contributions from interest group i depend on the level of services provided, where service P_i is a function of the effort E_i devoted to it. For example, E_1 is the legislator's effort devoted to serving the first group, $P_1(E_1)$ is the policy outcome the legislator can produce by E_1, and $R_1[P_1(E_1)]$ is the amount of contributions from group 1 the legislator would receive if he produces $P_1(E_1)$. If group 1 is the Sierra Club, E_1 might represent time spent in committee on an Acid Rain Prevention Bill; $P_1(E_1)$ the impact the legislator has on the bill through this effort; and $R_1[P_1(E_1)]$ the campaign contributions the legislator expects to receive from the Sierra Club as a result. The identity (3) claims that the total campaign resources the candidate receives are simply the sum of the resources contributed by each of the n interest groups. The assumption that the unorganized group contributes no campaign resources is reflected in equation (4).

In summary, the first five equations represent the assumptions of the model. The analysis is simplified greatly by dividing the activities of voting and of contributing campaign resources. Voters do not make contributions, and interest groups do not vote in this model. Interest group contributions are determined only by the promised level of effort of the legislator in the single dimension the group seeks to influence; there are no multiple-issue groups in the model.

The last function requires more careful scrutiny. Intuitively, equation (5) tells us that the unorganized group votes in response to three separate influences. First, effort devoted to producing constituency services, P_U, affects their votes. Next, the unorganized group responds also to all other policies 1 through n. That is, an individual, though not a member of the Sierra Club, may have strong preferences over the Acid Rain Prevention Bill. The vote cast by that individual may be affected by the legislator's activities in that direction, be it writing the bill, debating, or casting a roll-call vote. Third, the unorganized group responds to advertising or campaign expenditures made from R, the total resources contributed by interest groups, assuming each candidate uses the resources he receives in the optimal way.

Having developed the response functions of the groups, the next step is to characterize the strategy of the legislator to maximize votes, given a limit, $\bar{E}$, on the effort he has to allocate. For our purposes, *effort* is simply the amount of the legislator's time, the time spent by staff, and the amount of office resources a legislator allocates to an activity, and $\bar{E}$ is the total amount of effort he has available to allocate. The problem, then, is to choose the $n + 1$ levels of effort so as to maximize

$$V[P_U(E_U), P_1(E_1), \ldots, P_n(E_n), R],$$

providing that

$$\bar{E} = E_U + E_1 + E_2 + \ldots + E_n,$$

(total effort equals the constraint $\bar{E}$). For simplicity, let this symbol denote derivatives: ′; let superscripts denote partial derivatives; and suppose there are but three groups, U, 1, and 2. The necessary conditions for a solution imply:

$$\begin{aligned}\lambda &= (V^U)P_U' = (V^R R_1' + V^1)\,P_1' \\ &= (V^R R_2' + V^2)\,P_2', \qquad (6)\end{aligned}$$

where λ is the *shadow price* of effort. The overall meaning of equation (6) is quite sensible: a unit of effort has the same marginal effect on total votes, regardless of which activity it is devoted to—the usual result in maximization problems. If $\bar{E}$ is large enough, effort has no alternative use ($\lambda = 0$) and the terms in equation (6) equal 0, meaning that the legislator gets every vote he possibly can. If the constraint is binding, then $\lambda > 0$, and devoting effort to one activity now has a positive opportunity cost in terms of votes foregone by not engaging in some other activity. In this case, the maximizing strategy is to devote effort to each activity until the marginal gains are all the same.[5]

Each term in parentheses in equation (6) is the marginal vote productivity of a type of policy—that is, the change in vote received in response to a change in policy. The last two are the most interesting: $V^R R_1'$ represents the vote-producing effects of a change in resources (V^R) multiplied by the total change in resources in response to a change in services to group 1 (R_1').[6] The term V^1 is the vote-producing effect of policy service to interest group 1. Each P_i', the change in the amount policy *i* produced with respect to the change in effort devoted to producing it, is a measure of the legislator's relative ability to produce policy services for each of the three groups. Thus, the equality of the four expressions means that the productivity of effort in producing outcomes multiplied by the productivity of outcomes in producing votes is the same, for all types of effort. In addition, each expression equals λ, reflecting the fact that devoting effort to one activity has the opportunity cost of not devoting that effort to some other activity.

The legislator's decision problem is to allocate effort, given the vector of effort productivities, so as to maximize the probability of winning the election, subject to the constraint that total effort expended, $\sum_{i=1}^{n} E_i$, is less than or equal to $\bar{E}$. There are two potential difficulties with this formulation: the question of how the limit is fixed, and the possibility that the legislator's staff provides much of the services that affect votes and resource contributions. Fenno (1978, p. 34) claims that

> time is a House member's scarcest and most precious political resource. If there is an exemplary congressional complaint, surely "there isn't enough time" must be it. In deciding how to spend his time . . . a member confronts his most difficult allocative dilemma. . . . Different representatives make different allocative choices and different allocative trades.

The setting of the effort level is beyond the scope of this paper. However, the existence of a binding constraint on the legislator's effort is not implausible. The implication is that the candidate's time has some opportunity cost and is not valued at zero at the margin.

Still, we must consider the question of staff effort. As Fenno points out (1978, p. 34), it is not true that

> "all" the congressman has is time and himself. The office carries with it a large number of ancillary resources—a staff, office expenses allowances, free mailing privileges, personal expense accounts. . . . Each congressman chooses how he will use these resources. The most significant of these choices involve the use of staff.

The choice of staff size, like the constraint of effort, is not treated here. Staff effort is included within $\bar{E}$, the total effort the legislator has available. In sum, legislators choose how much time they themselves will spend working for reelection, and how large a staff they will retain once this choice is made. All of the available effort is allocated in such a way as to maximize the likelihood of reelection.

Choice of Constituency

It is important to understand the definitions of *constituency* used in this paper. The usual definition is those groups providing votes or campaign resources to the legislator. However, this definition does not really identify the choices available to the legislator in the decision about which set of groups to serve actively. There may well be groups that vote for, or donate resources to, the legislator, even though he does not actively serve them, and so these groups are not in his "constituency." In order to identify and separate the various constituencies, we will use the following definitions:

a) The *geographic constituency* is that part of the unorganized group of voters which votes for the legislator (see Loewenberg and Patterson, 1979, pp. 170–72; Fenno, 1978, pp. 1–8).
b) The *service constituency* is that set of groups the legislator actively devotes effort to serving.
c) The *resource-supplying constituency* is that set of groups supplying the legislator with campaign resources.

Notice that these are not necessarily the same: interest groups provide no votes, but may be served in exchange for resources; voters may not be actively served, but may vote for the legislator anyway. Further, interest groups may choose to offer resources to a legislator simply because of his views. Such a legislator receives resources not because of any expected quid pro quo, but because the group would like to see him stay in office.

Let $\hat{E} = (\hat{E}_U, \hat{E}_1, \hat{E}_2)$ be a solution to the three-group maximization problem. Suppose that, in addition to serving group U, the legislator also provides policy services to group 1, but not group 2. Notice that since the heterogeneous voter group responds to all policy dimensions, the *unorganized group* can affect the legislator's decision on whether or not to provide policy service to *interest groups*.

Let us examine next the case where one group is served and another is not. For example, if, contrary to equation (6), $\hat{E}_1 > 0 = \hat{E}_2$, we know that:

$$(V^1 + V^R R_1')P_1' > (V^2 + V^R R_2')P_2' \quad (7)$$

(policy service effort for group 1 produces more votes than it does from group 2). There are several cases where this might occur, the simplest of which is an absolute advantage in producing policy for group 1 ($P_1' > P_2'$, or, the marginal productivity of the legislator's effort is greater for P_1). That is, the legislator has some extra ability to produce P_1 as a committee member or chairman, a leadership position in the Congress. Also, he may have more knowledge about group 1 policy service because of prior experience, the location of his district office, etc.

If the legislator has no such advantage ($P_1' = P_2'$), equation (7) reduces to

$$(V^1 + V^R R_1') > (V^2 + V^R R_2') \quad (7')$$

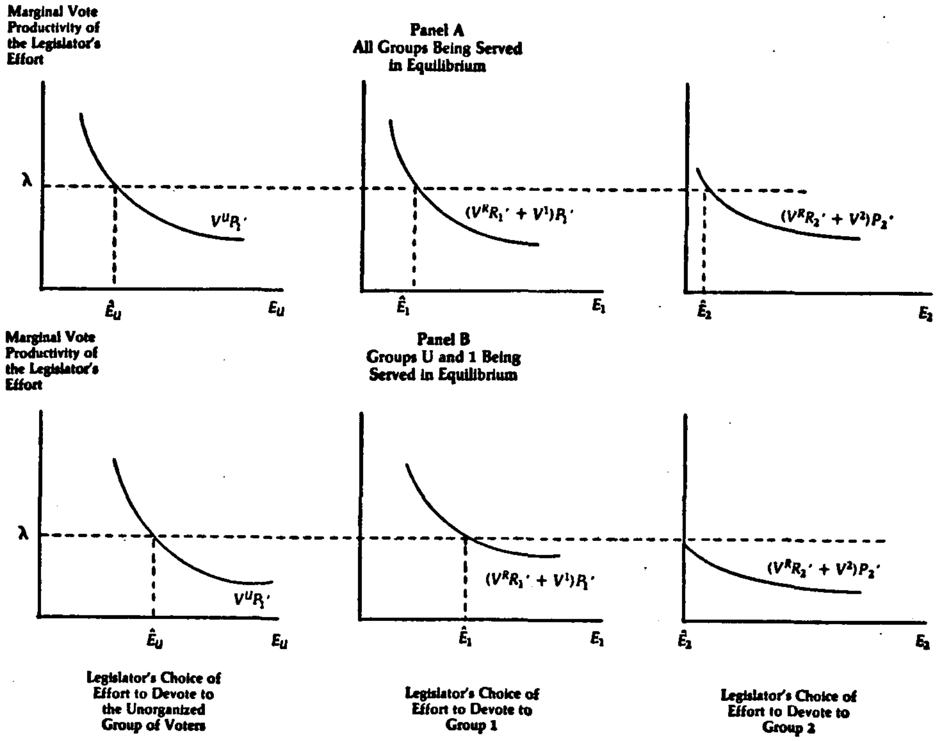
Each of the remaining cases will be examined separately.

($V^1 > V^2$): The voting response of the unorganized group is greater for policy 1, completely separate from the interest group response in providing resources. Thus, in deciding which interest groups to serve, the legislator must consider the response of the large, unorganized group.

($R_1' > R_2'$): Interest group 1 will provide more campaign resources in response to policy service than will group 2. In this simple case, we are assuming that there is no backlash from the unorganized group against a legislator who accepts campaign contributions other than that summarized in V^1, which can be large and negative. That is, the unorganized group makes its voting decisions based not on whether the legislator accepts money, but on what he does in order to elicit the contributions in the first place. The implication is that $V^R R_1' > V^R R_2'$, or that the votes the legislator can get from the unorganized group by advertising, multiplied by the marginal resource response he gets by producing policy services, is greater for group 1 than for group 2.

We can graphically depict the overall decision process of the legislator in choosing which group(s) to serve, assuming no two interest groups seek to influence the same policy. This graphical solution is shown in Figure 1, Panel A: at a maximum, the marginal vote productivity of each type of effort must be the same, and it must equal λ, the opportunity cost or shadow price of using effort for an alternative. In Panel A, all groups are served with positive levels of effort, but this need not be the case. if the marginal vote pro-

Figure 1. Legislator's Choice of Effort Allocation



ductivity of serving some group is very low (always less than λ, the opportunity cost of using that effort somewhere else), then that group will not be served. This situation is depicted in Figure 1, Panel B, where the legislator provides no policy service to group 2. The previous discussion showed that this can occur when (a) the legislator is not very good at producing P_2, whether because he is not on the "right" committee, is not knowledgeable, etc.; (b) because the unorganized group is indifferent or opposed to policy 2, offsetting the benefits from providing it; (c) interest group 2 provides few campaign resources in exchange for policy 2.

A Special Interest Group's Choice of Legislators

Thus far, we have modelled interest groups as supplying resources and votes in exchange for the policy and constituency services the legislator can provide. However, interest groups can be viewed as active seekers of those services from legislators. If interest groups are not passive, but actively seek legislators whom they believe can help them, then the situation looks quite different from that of the previous sections.

Suppose each interest group desires to achieve a set of goals, and makes its campaign resource contributions in the way it believes best accomplishes these goals. For example, the group may simply wish to have a legislator who will sponsor a bill when asked, which may be more effective if that legislator is on a particular committee or subcommittee. Alternatively, the group may want to be the first to know when legislation inimical to its interests is being considered. The group might want to be able to block or amend legislation. For other groups, it might suffice to be certain that the "right" people will be called to testify at committee hearings.

Recalling the earlier formulation, the maximization problem from the point of view of the legislator is: Maximize

$$V[P_U(E_U), P_1(E_1), P_2(E_2), \ldots, P_n(E_n), R]$$

subject to

$$\bar{E} = E_U + E_1 + E_2 + \ldots + E_n.$$

The first order conditions of this problem relating to interest groups are:

$$(V^R R_i' + V^i) P_i' - \lambda = 0, \quad i=1, \ldots, n. \qquad (8)$$

From the earlier discussion, recall that the terms inside the parentheses are the vote productivities of the n types of policy service. P_i' is then the productivity of effort for each policy. If we again consider only a single legislator, these expressions can be manipulated to illustrate the minimum price the legislator will accept to provide a unit of effort. This is done by solving for R_i' in the above equation. Given the usefulness of campaign resources (V^R), the marginal productivity of effort (P_i'), the reaction of the voters (V^i), and the opportunity cost of effort (λ), the quantity of resources a legislator will require to provide one unit of effort to the ith group is:

$$R_i' = \frac{\lambda - P_i' V^i}{V^R P_i'} \ . \qquad (9)$$

This expression can be separated into two parts:

$$R_i' = \underset{(a)}{\frac{\lambda}{V^R P_i'}} - \underset{(b)}{\frac{V^i}{V^R}} ; \qquad (9')$$

Part (a) of expression (9′) is fairly easy to understand. Since λ is constant across interest groups, and assuming V^R is constant (voters everywhere react the same way to advertising), only P_i' differs across legislators. Remember that P_i' is the marginal productivity of the legislator's effort in policy service to group i. Since P_i' appears in the denominator, the better the legislator at producing the ith policy service (i.e., the fewer hours he must devote to producing the same amount), the lower is the minimum price he will accept to provide that service. If we compare the legislator's price for services to groups 1 and 2, and look only at part (a), then if $P_1' > P_2'$ (the legislator is better at producing P_1 than he is at producing P_2) we have

$$R_1' = \frac{\lambda}{V^R P_1'} < \frac{\lambda}{V^R P_2'} = R_2'.$$

The more adept the legislator is at producing policy services for a group, the lower the minimum price he will require for doing so.

Part (b) adds an additional consideration; it is the ratio of the vote productivity of P_i to the vote productivity of resources. The larger this ratio, since it is subtracted, the smaller the supply price of that policy. Intuitively, this is what we would expect. The greater the voting response of the unorganized group to policy i, the fewer resources the legislator will demand to produce policy i. We might think of legislators whose geographic constituency is in favor of P_i as having a comparative advantage in producing P_i, at least from the point of view of interest groups.

Alternatively, if the unorganized group is very much opposed to policy i (that is, if $V^i < 0$), then this raises the supply price. Thus, the disadvantages of serving unpopular interest groups by producing policy helpful to them is built into the model.[7] Only if the interest group can provide large resource contributions will publicly unpopular policy service be provided.

A concrete extension of the problem of the interest group's choice of legislators is the case in which the interest group hopes to build a majority coalition of some group (a committee, subcommittee, etc.). If, for example, group 1 hopes to get a bill past the committee stage, it would form a ranking of the supply prices of the members of the committee. If there are k committee members, the ranking is from 1 (the lowest price) to k (the highest price). Notice that some of the lowest prices may be *negative*, meaning that the unorganized constituency of that legislator is so much in favor of that bill that he would have to be paid not to vote for it. Clearly, group 1 need allocate no resources to these legislators, because their support is "free." Beginning at the first price above zero, group 1 pays this price, and the next, until it has secured a majority. Suppose for example that there are seven committee members, and the supply prices are ranked as follows:

Legislator	*Supply Prices*
1	-$5000
2	- 2000
3	- 500
4	100
5	750
6	1500
7	2000

A majority on this committee can be had for $100; only this contribution to the fourth committee member need be made in order to gain a majority and ensure passage through the committee.

Unfortunately, the example thus far is flawed in an important respect: it assumes there are no competing interest groups, so that interest group 1 can act as a monopsonistic purchaser of policy services. Suppose there is a diametrically opposed group 2, so that $P_1 = -P_2$. That is, any policy service favorable to group 1 is unfavorable to group 2. For example, suppose the committee is the House Interior Committee, group 1 is the Sierra Club, and group 2 is the Chemical Manufacturers Association (CMA). The supply prices are the minimum the legislators will accept to provide P_1 or P_2; the actual price paid is likely to be bid up by the two competing groups.

To illustrate this, suppose the payoffs are the same as above, and that the committee must either report the bill to the floor ($P_1=1$, $P_2=-1$), or not report the bill ($P_1=-1$, $P_2=1$). Then the ranking is as follows:

Legislator	*P_1 supply prices (lowest to highest)*	*P_2 supply prices (highest to lowest)*
1	-$5000	$5000
2	- 2000	2000
3	- 500	500
4	100	- 100
5	750	- 750
6	1500	- 1500
7	2000	- 2000

If group 2 did not exist, then group 1 could buy $P_1=1$ for $100, because then L_1, L_2, L_3, and L_4 would vote for it. If group 2 did not exist, then the CMA could buy $P_2=1$ for free, because L_4, L_5, L_6, and L_7 all would have to be paid not to vote that way anyway. If, however, both groups do exist, and actively bid, it is quite conceivable that the legislators who are relatively indifferent (L_4, then L_3 and L_5, and so on) will receive a considerable amount of campaign resources.[8] The point of this example, however, is that although the unorganized group appears nowhere as an explicit agent in the bargaining over the policy outcome between the legislator and interest groups 1 and 2, the preferences and expected voting reaction of the unorganized group are implicitly embodied in the schedule of supply prices above, as we saw from (9) and (9′).

The results of this section can be summarized as follows. Interest groups choose among legislators based on the supply price of the service each group seeks. The supply price is determined by the "cost" to the legislator of providing the service. This cost depends first on the productivity of the legislator's effort. Productivity is affected by committee assignments, seniority, and the legislator's skill in using legislative institutions. Thus, the first component of cost is whatever the legislator gives up by serving an interest group and not using his time on something else. The second component of cost reflects voter preferences. A legislator seeking reelection must be compensated for votes lost by serving an interest group before he will provide such services. The amount required varies with the distaste of voters for the policy. In sum, the supply price or cost of policy to an interest group varies across legislators. Legislators have a comparative advantage in supplying policy on issues in which either (1) their effort is highly productive, or (2) their own voters are relatively more favorable toward increases in the policy. The following section illustrates a comparative statics implication of the model, based on the reactions of voters to changes in policy.

Rational Ignorance vs. Civics Class/Full Information

The discussion to this point has been concerned with legislators and interest groups struggling to achieve their own goals, and the ways in which their success can be affected and constrained by the large unorganized group of voters. No dominance or control has been ascribed to any of these agents; instead, the general framework encompasses the possibility that any one group or more may have the major influence on policy outcomes. In the context of the model, outcomes are determined by the prices, or costs, at which policies are offered by legislators. In order to illustrate the comparative statics predictions of the model, this section considers the supply price of policy under two alternative extreme assumptions about voter behavior. The two assumptions we consider are (a) rational ignorance, and (b) civics class/full information.

Rational Ignorance

One possibility for voter behavior is that voters invest very little in political information gathering. Resource contributions are the primary vehicle for determining policy outcomes. Voters in the unorganized group are ignorant of the activities of legislators in serving interest groups. According to this view (Becker, 1976, p. 37):

> since each person has a fixed number of votes—either 1 or 0—regardless of the amount of information he has and the intelligence used in acting on this information, and since minorities are usually given no representation, it does not "pay" to be well-informed and thoughtful on political issues, or even to vote . . . candidates for many offices, such as the presidency and state governorships, must have enough resources to reach millions of voters. Many groups that would like to compete for these offices do not have sufficient resources to reach large numbers of voters.

Thus, legislators must depend primarily on interest groups to provide them with enough resources to be able to make themselves known to the uninformed voters. By actively serving interest groups, legislators ensure interest group primacy in policy. Because voters are completely ignorant (until provided with biased information—Calvert, 1982), legislators can use these resources to advertise themselves with little concern about adverse voter response.

In the context of the model developed in this paper, *rational ignorance* implies that voters respond only to resource expenditures, because they are "rationally ignorant" of everything else. Thus, since only resources can mobilize votes, each legislator maximizes the resources he receives from interest groups, each interest group chooses legislators to contribute to on the basis of their policy productivity alone, and voters respond passively to advertising expenditures. The response functions of the agents look as follows: for voters,

$$V = V(R)$$

(policy has no effect; votes exchange for resources); for special interest groups,

$$R_i = R_i[P_i(E_i)],\ i = 1 \text{ to } n$$

(payment for legislator's effort); and for legislators,

$$R_i' \geqslant \frac{\lambda}{V^R P_i'} - \frac{V^i}{V^R} = \frac{\lambda}{V^R P_i'}$$

(because $V^i = 0$; no voter policy response). Only P_i' differentiates legislators, because λ and V^R are assumed constant. Notice that, as before, voters do not make contributions and contributors do not vote.

In this model, interest groups can almost completely control the legislator's activities. Those who act to further what they believe are the interests of their geographic constituency are never reelected, unless it happens that some interest group is in favor of the policy as well, and contributes resources. Legislators who act against the interests of their geographic constituency, but in accordance with interest group wishes, can lose an election only if the challenger has a higher policy productivity of effort in those policies, and can thereby win resources away from those less able, or with a smaller endowment of valuable committee assignments. The predictions of the model for desired committee assignments and voting patterns are quite simple (see Peltzman, 1982): assuming voters are rationally ignorant, both committee assignments and voting patterns chosen by legislators will be those which maximize the campaign resources received. In fact, the best strategy for a legislator is to keep his service constituency and his geographic constituency completely disjoint, so that voters are never served at all, unless they benefit from interest group resources spent in their districts.

The rational ignorance assumption model provides an interesting benchmark in extreme interest group influence, but is not a very useful view of the world. Notice that if voters are not ignorant about a legislator's policy activities in any policy category, interest group influence is affected. Putting this point in the context of the price an interest group must pay a legislator to provide effort on its behalf, let us reproduce (9′):

$$R_i' \geqslant \frac{\lambda}{V^R P_i'} - \frac{V^i}{V^R}. \qquad (9')$$

Voter's policy preference appears in the V^i term, the change in vote received in response to a change in policy *i*. If, for some geographic constituency, this term is negative (voters are not ignorant and are violently opposed to this policy), the interest group may find it either very expensive or impossible to purchase that legislator's services. This point can be extended to account for the role of the media or potential competitors: even if voters are currently ignorant of the activities of a legislator in serving an interest group, the legislator's actions may be constrained by the knowledge that the media or a competitor will expend resources to make voters aware. That is, the legislator must consider not only the reaction of voters given their present knowledge, but also the expected reaction if voters were to find out. The role of potential competitors and the news media in policing the actions of incumbents in this way has been noted by Mayhew (1974) and others, including MacKuen (1981, 1983).

Thus, another consideration enters the interest group's choice of which legislators to contribute to—the preferences of a legislator's geographic constituency.

Legislators whose geographic constituency has strong, informed preferences about some policy will receive resource contributions from an interest group only if voters and the interest group want the same thing; otherwise, the interest group will search out some other legislator whose geographic constituency is either ignorant or indifferent (Weingast and Marshall, 1984). Further, given the incentives for challenges and the media to monitor and report on the activities of legislators, the ignorance of the typical voter may still provide little room for discretionary behavior on the part of the legislator. In any case, the empirical prediction that the service constituency and the geographic constituency are disjoint is simply wrong: the intersection of the two groups is exactly those voters who are informed, have policy preferences, and who vote. The size of this group is an empirical matter; it is probably wrong to suggest a priori, as Becker (1976) seems to, that the group does not exist.

Civics Class/Full Information

The *civics class* model of voter behavior developed in this section is conceptually the opposite of the rational ignorance model just described.[9] The overriding concern of the legislator, under the assumption that voters act the way they are depicted in a high school civics class, is to discover the preferences of what we have called the geographic constituency, and then to act so as to serve these preferences. As shown below, interest groups have very little influence in such a scheme. Fully informed voters imply a model of legislative behavior where the service constituency and the geographic constituency are identical; interest group service is at most an artifact or coincidence resulting from the preferences the legislator is actually serving.

If voters are well-informed and have strong preferences, advertising has little effect, so that V^R, the partial derivative of the vote function with respect to resource expenditures, is near zero. The response functions are then as follows: for voters,

$$V = V(P_U, P_1, \ldots, P_n, R);$$

for special interest groups,

$$R_i = R_i[P_i(E_i)], \; i = 1 \text{ to } n;$$

and for legislators,

$$R_i' \geqslant \frac{\lambda}{V^R P_i'} - \frac{V^i}{V^R}.$$

V_R goes to zero, and R_i (the legislator's minimum supply price) becomes very large, moving toward either positive or negative infinity. No legislator can be induced to serve an interest group he would not already be serving. But this is an obvious consequence of our assertion that legislators maximize the number of votes they receive, and that effort is limited. If advertising resources produce no votes, and the legislator is prevented from using these resources for his own consumption, then he will not devote any effort to obtaining resources.[10]

Implications of the Models

Having illustrated the implications of the alternative assumptions of complete voter ignorance and full information, it is possible to address the question of the behavior implied by each. First, if voters are ignorant, it is clear that preferred committee assignments for legislators bear no resemblance to geographical constituency interests. Seniority, committee chairmanships, etc., are important because of their value as capital assets, capable of generating resource flows that are indirectly translated into votes through advertising. If voters are informed, however, there are very different implications for committee preferences. The skills of the legislator, prior knowledge, and other factors are of secondary importance. The primary goal is a committee position which enables the legislator to serve his geographical constituency. Interestingly, however, seniority and committee leadership positions are still very valuable, even if they are not capital, or resource-producing, assets: these things can provide votes directly, because they increase the effectiveness of the effort devoted to serving the geographical constituency.[11]

The behavior of interest groups can be compared by examining the supply price of a legislator implied by each assumption. Uninformed voters imply that a legislator's willingness to exchange effort devoted to legislative services for resources is affected only by the response of his geographic constituency to advertising, the opportunity cost of effort, and the productivity of the legislator's effort:

$$R_i'{}_{(\text{uninformed})} = \frac{\lambda}{V^R P_i'}.$$

In this model, interest groups determine policy outcomes by bidding for a legislator's effort.

Interest group behavior under full information (assuming $V^R=0$, or that advertising is ineffective) is quite different. A legislator's supply price, R_i' (when voters are fully informed) is simply:

$$\lim_{V^R \to 0} \frac{\lambda}{V^R P_i'} - \frac{V^i}{V^R} = \pm \infty$$

(depending on the sign of $\lambda - P_i'V^i$), and thus interest groups can have no independent effect whatsoever. Interest group preferences and offers of resources have no impact on legislators' behavior.

Thus, neither assumption is completely consistent with the two most noted "facts" about the relations among legislators, special interest groups, and geographic constituencies. First, legislators choose committees which have jurisdiction over matters important to the geographic constituency (Niskanen, 1971; Weingast and Marshall, 1984), contrary to the predictions of the rational ignorance assumption and consistent with full information. Second, as Dexter (1956, 1963) points out, interest groups choose legislators whose geographic constituencies have similar demands. Legislators elected from homogenous geographic constituencies will value committee assignments that are of direct importance to their voters: e.g., legislators from Kansas seek seats on the Agriculture Committee. To the extent that legislators elected from heterogeneous geographic constituencies need more resources in order to advertise, these legislators will pursue committee seats that help them affect legislation wealthy interest groups will notice. That is, optimal election strategies must vary at the margin with the composition and preferences of the voters in the geographic constituency.

Interest groups have a complicated problem in seeking out the legislator with the lowest supply price. However, as we have demonstrated, an interest group need not search very far for legislators with relatively low supply prices of effort, because those whose geographic constituencies are most in favor, or least opposed to, a policy are likely to be the cheapest source of interest group legislative services. The result, then, is that interest group policy manipulation is constrained by the preferences of the geographic constituency.[12] This constraint is imperfect, however, because voters are less than perfectly informed. Legislators themselves act in ways that do not perfectly represent their geographic constituency, in order to gain campaign resources from interest groups; these resources are used by each legislator both to portray himself in an attractive way and to inform voters. The voting group is not "organized" in Olson's (1965) sense, but legislators acting as political entrepreneurs can use the unorganized group to countervail interest group pressure (Wagner, 1966, p. 164).

The conclusion is that interest groups

do not control this process, but neither are special interests powerless. Contributions can have some influence on policies about which voters are divided, ignorant, or indifferent. The geographic constituency is not represented to the exclusion of all other groups, but departures by legislators from their voters' interests are constrained by the strong preferences voters have on some issues, and by the threat of informing and mobilizing public opinion that the news media and potential competitors always represent. In short, each of the extreme information assumptions offers a useful view of a small portion of the legislative process some of the time. Yet each is only part of a larger view of the complicated process exhibiting hidden, but very real, incentives that impel the activities of the political agents.[13]

Summary and Conclusions

This paper presents a model of the supply of government policy. The agents in the model include legislators, voters, and interest groups. The supply price, or cost, of policy is derived assuming that legislators seek to maximize votes, which can be obtained either by providing policy that voters favor or by serving interest groups in exchange for vote-producing campaign resources.

The price of policy for an interest group is shown to depend on the comparative advantage of the legislators offering to help provide the policy. The two factors determining comparative advantage are productivity and the preferences of the voters back home. Specifically, the more productive a legislator's effort, or the less hostile that voters are to a given policy, the lower the minimum price an interest group must pay in exchange. Since legislators differ in their productive capabilities (because of differences in committee assignment, seniority, and competence), and since the voters in their geographic constituencies differ in their preferences, these prices vary in predictable ways across legislators. Interest groups can be expected to choose among alternative legislators on this basis.

Finally, an explicit comparative statics result of the model is derived. We ask under what circumstances will unorganized interests (i.e., voters) be represented. Two extreme alternative informational assumptions are considered. First, voters are assumed to be completely (though rationally) ignorant of the effects of a given policy on their welfare. The implications of this assumption are contrasted with a situation in which voters are fully informed. The supply price observed is demonstrated to differ, in a predictable way, between these two situations. To the extent that any voters are informed and opposed to a policy, an interest group seeking favorable treatment must pay a higher price than if all voters were indifferent or uninformed.

The conclusion is that unorganized, noncontributing voters may be effectively represented even in a situation in which interest groups are well organized and active. The representation of such voters, deriving from the institutional requirement of periodic reelection for legislators, tends to reduce the influence of organized but nonvoting economic interests in the political process. Thus, unorganized groups can shape and constrain decisions to a greater extent than that predicted by simple demand-oriented group theories of collective action.

Several extensions of the model presented above merit future consideration. The model is capable of addressing comparative statics questions such as the effects of changes in committee assignments or jurisdictions, or of changes in district boundaries. Committee assignments influence the policy productivity of legislators; redistricting modifies the vote functions that influence legislator and, implicitly, interest group decisions.

In addition, the model itself could be

extended in at least two ways. First, the source of the vote functions, or what others have called "political support functions," can be explicitly derived using a model of consumer voter behavior similar to that of Denzau and Parks (1979).[14] Second, the equilibrium properties of the model, with voter preferences more clearly defined, can be explored in the context of a formal game to illustrate the implications for policy of interest groups competing directly against one another.

Notes

The authors wish to thank Randall Calvert, William Dougan, Brian Humes, William Riker, Kenneth Shepsle, and Barry Weingast for helpful comments and suggestions.

1. Fiorina (1974, p. 48) demonstrates that when election is "assured," votes are not maximized if candidate resources have an opportunity cost. However, assuming the candidate hopes to win at least two elections, and that the constraint on effort accounts for opportunity cost, the election is not assured in Fiorina's sense; legislators in our model are "maximizers" rather than "maintainers."

2. At a more detailed level, this outcome is itself the outcome of a game involving members of the interest group. The very existence of one pattern or set of interest groups is an equilibrium of a complicated process that we take as exogenous. We will allow, however, for the possibility that an interest group has zero members, or that no organized group actually exists, for the reasons Olson (1965) discusses.

3. A model using assumptions similar to those described here has been developed by Cox, McCubbins, and Sullivan (1983). Using the reelection motivation for legislators and Fenno's taxonomy of constituent groups, they examine technological constraints on legislators' ability to choose forms of public policy implementation that improve their reelection chances. The orientation in the present paper, though arising from the same assumptions, is on the constraints placed on legislators' and interest groups' activities not by "technology," but by voters through the electoral mechanism itself.

4. The fact that interest groups provide no votes is not crucial to the results presented here. An earlier version of this paper included interest group voting, adding considerable complexity, but no qualitative difference in the results. Interest group voting was thus dropped for expositional simplicity. Dougan and Kenyon (1984) present a model in which interest groups provide both money and votes, and they obtain results qualitatively similar to ours.

5. Suppose this were not true, and that the last hour of effort devoted to service to the unorganized group produces 9 votes, while that devoted to group 1 produced only 6 votes. Then the legislator could increase his total votes by working one less hour on P_1 (he loses 6), one more on P_0 (he gets 9), so that there is a net gain of 3 votes.

6. This formulation is used for notational simplicity. Since $R = \Sigma R_i$, but only R_i is affected by a change in E_i, R_i' is the change in total resources in response to a change in the services provided to group *i*.

7. From the first order conditions, we can explicitly derive the conditions under which the candidate will "sell out." If $V^R R_i' P_i' > \lambda - V^i P_i'$, then $\hat{E}_i > 0$. Suppose $V^i P_i' < 0$ (geographic constituency is opposed). Then if serving group *i* produces more votes (through use of resources gained by $\hat{E}_i > 0$) than the sum of (1) the opportunity cost of legislator's time, and (2) the negative reaction of voters to policy *i*, the legislator will act directly against the interest of his geographic constituency. We would expect to see this occur when (a) the interest group being served is willing to pay large amounts, and (b) the legislator's marginal vote value of resources is high. Thus, poor campaigns are more likely to serve interest groups, to the detriment of voters.

8. William Riker points out that this example bears a striking resemblance to the reasoning used by Shapley and Shubik in deriving an abstract "power," or value index for use in predicting outcomes in small numbers bargaining games (see Schotter and Schwodiauer, 1980, for a review of the development of this concept, and for references). While a game theoretic treatment of the bargaining between interest groups and legislators is beyond the scope of this paper, the similarity in results suggests both the generality and usefulness of Shapley and Shubik's result, and an interesting direction for further research.

9. Fenno (1973), and Shepsle (1978) portray legislators as seeking committee assignments that tend to further their personal goals, one of which is reelection. Since we have abstracted away from nonelection goals of legislators, this view is not here incorporated. A more sophisticated treatment, with legislators trading off votes against the ability to act on their own preferences, given some chance of reelection, could be incorporated as an extension of the present model (see note 1 above). For our purposes now, however, Fenno's (1978) description of a legislator's behavior is what we assume legislators believe about voters: that each has strong and informed preferences. In the present model, this problem is ignored: the $V(\cdot)$ functions are known. Clearly, an extension of the model is to allow some combination of the two extreme informational assumptions made in the text, perhaps allowing preferences to be unknown to the legislator, or else associated with some stochastic function.

10. The consideration of "political advertising" given here is at most a preliminary treatment of a complicated problem. If, as has been suggested in the economics literature (Benham, 1973, and other articles reviewed by Comanor and Wilson, 1979), advertising does not persuade, but instead activates or informs existing latent preferences, then the usefulness of campaign resources depends crucially on what is being advertised. While this effect is implicitly built into the present model, the role of advertising on outcomes requires a more sophisticated theoretical treatment than that developed so far.

11. Weingast and Marshall (1984) suggest that the present system has evolved because of the recognition by legislators that political "gains from trade" can be realized. Drawing on the industrial organization literature, Weingast and Marshall suggest that the costs of enforcing impicit contracts between voters and legislators create incentives for legislators to seek the "best" committee assignments out of personal self-interest. Earlier versions of our paper included a rather long section on committee assignments and optimal assignments rules. However, the section was removed for reasons of length. For an application of the idea, see Munger (1985).

12. Mancur Olson (1965) demonstrates that opposing interest groups, either existing or potential, cannot be expected to constrain interest group activity. The "free rider" problem arising from the shared benefits of organizing opposing interest groups implies the interest equilibrium of pluralist theory may well not occur at mean or median of all preferences; some groups will benefit at the expense of others, even if those others are partially organized.

13. An empirical implication of the model is that if it is cheaper and more effective for an interest group to try to affect those policies that the unorganized voters are in favor of, we would expect a pattern to develop over time in the growth of an interest group. A nascent interest group with a poorly developed resource base will try to affect those policies that are publicly popular and widely supported in the jurisdictions in which it organizes. As the interest group grows in the quantity of resources it can command, it should then begin to concentrate on less popular policy directions that provide more benefits for interest group members. In sum, interest groups may be public policy-oriented at the outset, but over time move toward more personal favors for interest group members. This proposition is to be developed and tested in future work.

14. The Denzau and Parks model expresses public sector preferences in terms of the private consumption possibilities set that they imply. The properties of these derived preferences appear quite tractable for incorporation within the present model.

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A POLICY-ORIENTED THEORY OF CORRUPTION

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Perspectives from political science and economics are drawn on to suggest an integrated theory of governmental corruption. The theory is oriented toward policy choices, and corruption is viewed as a product of individual and structural variables that interact to produce both positive and negative consequences. Individual-level considerations such as greed and the likelihood of detection and prosecution suggest one set of policies for reducing corruption. Bureaucratic constraints, citizen participation, and the congruence of legal structures and social demands offer a competing set of concerns that must be dealt with in analyzing corrupt practices. We show that corruption, as a process, influences the optimal level of social welfare. Alternative conceptions are examined, and a model is developed to evaluate policy choices related to corruption from the perspective of welfare optimization. Applied to the analysis of corruption, the model integrates general equilibrium theory, deterrence theory, and structural conditions. Finally, policy implications are considered.

Despite its frequent occurrence, governmental corruption has undergone surprisingly little systematic investigation. The theoretical literature has adopted either broad classifications of administrative corruption (Heidenheimer, 1970) or categorical definitions of a wide range of corrupt activities (Peters and Welch, 1978), and has relied largely on descriptive analysis in examining the causes and effects of corrupt behavior. As Werner (1983, p. 152) notes, "If the field of administrative corruption is to become more theoretical and less descriptive, it must develop a framework and methodology that will permit comparative analysis." There remains a need for an internally consistent theoretical model and an analytical definition which would lead to important policy considerations.

This article examines corruption from the combined perspectives of political science and economics to develop a theoretical framework and methodology for policy analysis. Political science tends toward anecdotal accounts of individual cases of corruption that are useful in defining the concept, but provide little systematic analysis. Economics views

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

corruption within a model of rational individual choice, with little concentration on the overall impact on society. While both disciplines provide insight into differing aspects of corruption, neither alone produces a sufficiently satisfying explanation of the pervasiveness of corruption in society. In this study, by incorporating both perspectives we develop a coherent theoretical framework from which the causes and societal consequences of corruption can be assessed.

The first section examines previous interpretations of corruption, develops a definition appropriate to an interdisciplinary approach, and provides a dichotomous conception of the effects of corruption as being either beneficial or detrimental. In the second section the effects of corruption on social welfare are placed in the context of a general equilibrium model. This is followed by a description of a formal model that integrates general equilibrium theory, deterrence theory, and structural variables relevant to corruption. Public policy implications derived from this model are then explored. The major implication of examining the problem in these terms is that society may need to alter substantially its approach to dealing with corruption.

Problems of Definition

One of the difficulties with this topic stems from the lack of a widely accepted definition of corruption. Heidenheimer (1970, pp. 3–9) describes three types of corrupt behavior definitions: public office–centered, market-centered, and public interest–centered. Public office–centered definitions revolve around the violation of the public trust placed in the official. Market-centered corruption refers to the situation in which the official sees the position as an authority to maximize personal gain by dispensing public benefits. A public interest–centered definition of corruption stresses the violation of the common interest in favor of special interests that provide direct or indirect benefit to government officials. In a similar vein, Peters and Welch (1978, p. 76) develop a four-part classification (official involvement, actual favor granted, payoff to official, and the donor of the payoff) in a survey of state senators to rank particular acts as more corrupt or less corrupt.

These classifications have merit in the description of a specific corrupt event, yet they lack the generality essential for an analytically useful definition of corruption. To fill this gap, our analysis employs a definition that incorporates alternative interpretations that have been developed in the literature. We define a corrupt act as any illegitimate use of public power or authority for private benefit. Such a concept subsumes all of the previous definitions and permits the analysis of a wide range of corrupt practices. It should be clear that this definition excludes "pork barrelling" and other forms of economically inefficient but politically attractive distributive policies, since they use legitimate (legislative bargaining) means to influence governmental behavior (Weingast, Shepsle, and Johnson, 1981).

A second problem with the literature on corruption is that it is largely descriptive and anecdotal. Sinclair's portrayal of the meat packing industry and Gardiner's assessment of organized crime in an American city are representative of the problem (Gardiner, 1970; Sinclair, 1908). While both works provide tremendous insight into specific cases of corruption, they provide little coherent or systematic theory concerning the topic.

What is lacking in most of the published material is a systematic mechanism for linking the causes of corruption with its consequences to generate public policy alternatives. Regardless of the debate over definition and outrage over specific examples of corrupt behavior, the question remains, "What should our society do about corruption?" To attempt an answer it is necessary first to examine the causes and consequences of corruption.

Causes of Corruption

The causes of corruption can be viewed from two distinct perspectives; one involves individual characteristics while the other concentrates on structural influences. The individual-level explanation tends to view the corrupt act as the result of greed or the inability to withstand temptations on the part of weak or insufficiently ethical officials. Greed, or desire for gain, is indeed a strong motivator of human behavior, as the literature suggests. The Knapp Commission's study of police wrongdoing in New York City classified corrupt officers as either "grass eaters" or "meat eaters" depending on the extent of their personal greed (Knapp Commission, 1973). Gardiner and Olson (1974, pp. 274–81) discuss the inability to withstand temptation and problems of individual value systems as two of the major explanations of corruption. Rogow and Lasswell (1963, pp. 45–54) classify corrupt machine bosses as having either "gain" or "game" orientations toward politics. While these individual explanations provide valuable insight into why some officials can become corrupt, they are not satisfying explanations for the extent of corruption in our society, because they lack general applicability.

In order to provide a broader level of analysis we explore three categories of structural-level explanations: (1) bureaucratic or organizational (δ), (2) quality of citizen involvement (γ), and (3) congruence of the legal system to social demands (π). The first two are derived from the existing literature while the third is described in this paper for the first time.

The organizational or bureaucratic approach to corruption has a number of variations, but the essential theme is the inability to accomplish public purposes due to impediments created by bureaucratic organization. One variant of this theme has been labeled the "bottleneck theory." The idea is that bureaucratic inertia can cause people to attempt to circumvent official channels through bribery or other illegitimate means to achieve a desired result (Johnson, 1982, pp. 22–24). Rose-Ackerman (1978, pp. 85–186) devotes nearly half of her book to examining how various bureaucratic arrangements influence the level of corruption.

The quality of political participation is also described as a deterrent of corruption. Nonpartisan ballots and at-large municipal elections were aimed at improving the quality of participation and breaking electoral support for ward politicians and political machines. In recent years, reelection of corrupt officials again raises questions about the quality of democratic participation (Rundquist, Strom, and Peters, 1977, pp. 954–63). If officials are not closely scrutinized, it would seem more likely that they would feel unconstrained in seeking their own benefit over that of the public.

A third structural view of corruption involves problems caused by the lack of congruence between the legal system and social demand, and can be seen in the cases of prostitution, gambling, and drugs. The law in the vast majority of the United States prohibits prostitution, gambling, and certain recreational drugs, yet at the same time there is a substantial demand for such services and products. If the legal system does not adequately respond to demands from the public or an intensely interested subgroup, then the likelihood of corruption increases. Officials are susceptible to being bribed to overlook violations that are desired by a segment of the public. In this situation, the political system, or more correctly, the lack of congruence between social demands and political outcomes, can be a cause of corruption.

A somewhat different problem concerning the legal system occurs if property rights are unclear, as it is possible for people to attempt to influence an official illegitimately in a cause in which they feel they are protecting their property. An example of this type of situation is in the illegal pollution of a waterway by a company. Ownership of a river may be unclear, while the company is clearly owned by the shareholders. The pressure would be on the company to dispose of hazardous waste as inexpensively as possible. An environmental official is required to safeguard the public's interest in the river, yet the interest is not clearly defined. It is easy to envision a situation where an official does not seek aggressive enforcement in exchange for private benefit from the company. If property rights concerning the river or other natural resources were more clearly defined it would be expected that the owners of the property would vigorously protest its violation and require enforcement of the laws.

Consequences of Corruption

In addition to the causes of corruption, an integrated public policy approach requires an examination of the consequences of such behavior, so that scarce governmental resources can be efficiently utilized. The bulk of the corruption literature has described the consequences as being routinely negative. A 1966 article, "Corruption, The Shame of the States" (Wilson, 1966), is illustrative of the traditional self-righteous approach to the topic. There are a few notable exceptions to this approach, such as Merton's (1972) analysis of the social service functions provided to otherwise ignored immigrants by apparently corrupt political machines. Similarly, Nye (1967) attempts to evaluate the possible positive consequences of corruption for third world economic development. Both studies attempted to evaluate the consequences of corruption rather than assuming the consequences were negative.

Debate over the effects of corruption on society has lately been revived. A recent study suggests that corrupt practices have a "spillover" effect on the remainder of society. The "spillover" takes a number of forms that lead to a lessened respect for the government on the part of the public. This effect is negative and is the putative consequence of administrative corruption (Werner, 1983, pp. 149–50).

To reduce the confusion over the effects of corruption upon the overall welfare of a society it is necessary to evaluate systematically the negative aspects of a corrupt activity in comparison with the advantages derived by the participants in the illegitimate exchange. With this approach it is possible to interpret corruption as having either a beneficial or detrimental net effect on society.

Beneficial Corruption

The concept of beneficial corruption is counter-intuitive and thus requires elaboration. When the production of desired goods and services in society is inhibited by structural inefficiencies (bureaucratic, legal, and political), corruption could serve as a means of attaining the desired outcome. The individuals participating in the illegitimate activity directly benefit from the transaction, while simultaneously raising social welfare by creating opportunities for production that would otherwise be restricted.

An example will be useful at this point. Prostitution is nearly universal, yet the activity is illegal in most locations. The demand for prostitution is at least extensive enough to raise large sums of money, which in turn generates lucrative bribes for vice control officers. The causes of prostitution are not clearly understood but are related to a number of societal conditions concerning sexual activity among consenting adults. Given the sexual revolution of the 1960s and 1970s, it may not be unreasonable to suggest that the laws prohibiting the performance of sexual activities for money lag significantly behind the actual level of tolerance in society. Assuming this to be the case, it is easy to see how a police officer could overlook his or her duty to enforce an outdated law because of a bribe or payoff.

The primary benefits in this example are derived by the customer of the service, the prostitute, the procurer, and the official who accepts the bribe. These benefits can be grouped into two categories: (1) the direct gain of the participants in the illegitimate transaction, and (2) the reduction of the likelihood of additional negative sanctions upon individuals performing a widely demanded service.

Possible externalities of corruption and equitable distribution of the benefits in society must also be taken into account. The negative externalities may produce long term disenchantment with the legal system and thereby harm society. Because these negative effects are distributed in a diffuse way with little direct damage to any particular individual, it is difficult to evaluate the extent of the harm.

The distribution of direct benefits in society is also an important determinant of the desirability of a corrupt transaction. If the outcome of the transaction differentially benefits an income group not requiring preferential treatment according to the distributive equity standards of the society, then the social desirability of the corruption would be lowered. In the case of prostitution it is difficult to determine the distributional impact with any specificity. One could argue, however, that the distribution resulting from corruption may be favorable because of the dual benefit that a prostitute can derive by being able to earn income and avoid imprisonment. Using a Rawlsian perspective, this distribution can be justified if it is assumed that the prostitute is among the least advantaged segment of society (Rawls, 1971). The distributional equity question must be examined individually for each situation in which beneficial corruption exists.

The idea of beneficial corruption does not imply that these activities should be considered legitimate, but rather that they provide a net increase in social welfare. What is implied is that there must be a serious structural flaw for any corrupt act to lead to increased welfare. This is where the policy alternatives that will be described later come into play.

Detrimental Corruption

The concept of detrimental corruption is easier to reconcile with the bulk of the literature on the subject; however, our interpretation requires further elucidation. A particular corrupt activity will be considered detrimental in this analysis if the net impact on society is negative. An example, concerning the bribery of a mine safety inspector, will help to illuminate our conception of detrimental corruption.

A mine safety inspector is required to certify that conditions in a particular mining operation are safe. The cost of actually maintaining a safe mining environment is extensive for the mineral extraction company. The company might cut costs on its safety equipment by bribing a safety inspector to incorrectly certify that the mine is safe when in fact it is dangerous. The company and the corrupt official would profit from the transaction, but the risk of disaster for the miners would be enormous. Even if the profitable transaction between the company and the official were to last many years before a disaster actually did occur, the loss of life resulting from a mine cave-in or explosion would exceed any monetary considerations. The situation would be categorized as detrimental in our analysis, regardless of the income transfer among the parties, because of the health risk caused by the corrupt transaction.

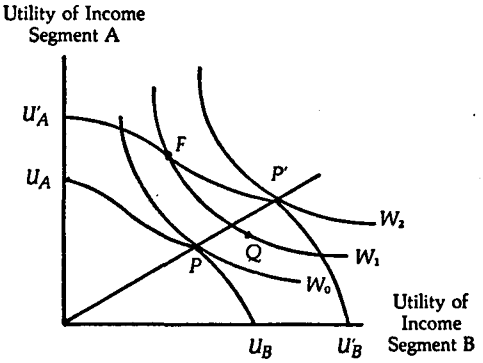
The conception of beneficial and detrimental corruption described above is useful in exploring the nature of corruption intuitively, but it is hardly systematic. To develop a systematic mechanism for analyzing the impact of corruption on social welfare and to derive public policy alternatives, this conception needs to be articulated within a general equilibrium framework, since such alternatives involve society's use of resources.

Corruption and Social Welfare

Consider a general equilibrium model that includes a social welfare function and the Pareto optimal conditions for the economy as a whole. In addition to the efficiency conditions most commonly stated for social welfare maximization, it will be assumed that society's production is organized in accordance with Pareto optimality; that property rights are sufficiently defined to assure every member of society the desired output mix; and that the interrelationship among production, consumption, and distribution is legitimized within a legal structure synchronized to the Pareto optimum setting.[1] The general feature of this model is that it extends the Pareto optimum and welfare maximum conditions to the organizational, political, and legal structures of a society. The public sector is assumed to act in direct response to society's preferences, and its involvement in the private market is limited to the conventional allocation, stabilization, and distribution functions that are largely exercised to maintain the Pareto welfare maximum.[2]

Under these conditions, any alteration in the public to private output mix will be assumed legitimate only if it is along the lines of the Pareto welfare maximum and is authorized by the prevailing legal system. In other words, changing a position on the Pareto welfare frontier will be a legitimate rearrangement if it results from a desired alteration in social preferences justifiable by the legal structure of the society; otherwise it will be illegitimate, which may dictate additional use of societal resources for its internalization.

Figure 1. Corruption and Social Welfare in a General Equilibrium Model



Key to Figure 1:

$U_A U_B$	is a welfare frontier representing a possible distribution of real income (utility) between two income segments *A* and *B*.
$U'_A U'_B$	represents the same distribution at a higher level of societal income.
W_0	is the Bergson-Samuelson social welfare function.
W_1 and W_2	the higher numbered curves represent society's distributional judgment at higher levels of social welfare.
P and *P'*	are allocations which simultaneously satisfy Pareto optimum and social welfare criteria.
F and *Q*	imply possible allocations and distributions which may result from beneficial corruption when society is initially at point *P*. Corruption will be detrimental if society can freely move to *P'* but is forced to move to *F* and *Q* or remain at *P*.

This is demonstrated in Figure 1, where $U_A U_B$ depicts the welfare frontier and *W* the Bergson-Samuelson social welfare function.[3] A line from the origin through point *P* represents the public to private output ratio most desirable distributionally. The allocation which corresponds to *P* simultaneously satisfies Pareto optimum and welfare criteria. Any deviation from this point would be a distributional (allocational if the welfare frontier moves outward) improvement only if it is desired by the society and accompanied by a sufficient alteration in the organizational and legal structure. Thus, as shown in Figure 1, a move from P to P' would be an acceptable and legal one. Under the given assumptions, any other position off the frontier, say point Q, would be inefficient and illegal, leading to welfare losses shown as the difference between W_1 and W_2 on the diagram.

To add realism to the model, let us assume that society encounters obstacles in adjusting its organizational and legal structures to the preferred changes in the output mix.[4] The public sector lags behind the private sector in adapting to new production technologies, and at the same time the legal structure fails to adjust quickly to the changing conventions, norms, and expectations of society.[5]

Given these changes, consider point P in Figure 1 and the following cases:

1. Society's preferences dictate an output mix other than P, yet the legal structure does not authorize such alteration;
2. Society is at point P, and, although the legal structure is permissive, there are bureaucratic obstacles which may delay a desired move towards another point on either U_AU_B or $U'_AU'_B$;
3. Society is at point P, and, despite inefficiencies in the organizational and legal structures, there is no tendency for society to look for another resting point on either frontier; and
4. Society can freely move from P to any other point on either frontier without being affected by organizational and legal inefficiencies.

In all four cases initiating forces will be present to move society to another resting point, either through legitimate or illegitimate means. The forces which use illegitimate means will become the sources of corrupt practices CP. This means that transformation of societal resources and alterations in the public to private output ratio will result from corrupt practices only if they are caused by an unauthorized collaborated effort between public and private sector producers. These practices correspond to the interpretation of corruption which is used in the current literature, and incorporate activities associated with the office-centered, market-centered, and public-centered types of violations reviewed earlier.

Note that the illegitimate means in case 1 and case 2 result in increased welfare in the Pareto sense only if the social change is on the welfare frontier and stays along the lines of the social welfare function. This would correspond to the notion of beneficial corruption presented above. In cases 3 and 4, illegitimate means will lead to welfare losses, which we define as detrimental corruption. It is likely that both welfare effects will be present in any corrupt practice. The determination of whether a particular corrupt transaction is beneficial or detrimental will be based upon the net impact on social welfare.

Beneficial corruption is viewed as a "good" in this model because of its positive net impact on society's welfare. Corrupt practices shift U_AU_B outward as indicated in cases 1 and 2, and restore a public to private output ratio at a higher welfare level shown by the tangency point P'. This point will be maintained only if society incurs no additional costs related to corrupt practices. When such costs exist, society will restore the equilibrium at a welfare level lower than P' but higher than initial point P, primarily because of the costs associated with the corrupt actions and the spillover effects on the rest of the economy. Consider, for example, the welfare losses $W_L(SC)$ that may result because of the external diseconomies (SC) associated with beneficial corruption, such as the negative implications following from the violations of the rules and norms of a society. Also, assume that society incurs additional losses $W_L(R_1)$, due to resource use in corruption prevention, and $W_L(R_2)$, due to resource use in the corrupt sector to conceal corruption and minimize punishment if apprehended.

When these costs are significant the welfare frontier moves inward, restoring a public to private output ratio at a lower level of welfare. For corruption to be beneficial under these conditions the welfare gains from corruption (W_G) must exceed the welfare losses (W_L). This means

$$\frac{\partial W_G}{\partial CP} > \frac{\partial W_L}{\partial CP}, \qquad (1)$$

or more specifically,

$$\frac{\partial W_G}{\partial CP} > \left[\frac{\partial W_L(SC)}{\partial CP} + \frac{\partial W_L(R_1)}{\partial CP} + \frac{\partial W_L(R_2)}{\partial CP} \right] . \qquad (2)$$

Note that alteration of public to private output ratios could lead to welfare gains, but at the same time change the distributive equity. As shown in Figure 1, when the initiating forces in cases 1 and 2 restore a Pareto-efficient allocation at point *F*, income distribution improves in favor of income group *A*, but at the same time reduces social welfare to the level represented by W_1. From an economist's perspective such changes in resource allocation are still beneficial as long as the new equilibrium *F* represents higher welfare than the initial allocation *P*.[6]

It should be emphasized that the magnitude of welfare loss due to distributional effects will depend on society's notion of fairness. From a utilitarian distributive equity approach, corruption will be beneficial as long as it adds to welfare despite its adverse impact on income distribution. An egalitarian will tolerate such corruption if it improves equality of income, and so will a Rawlsian, as long as the utility of the least advantaged group in society is maximized. Under each of these equity assumptions, the rule for beneficial corruption is still valid: corruption is beneficial as long as welfare gains exceed welfare losses; otherwise it will be detrimental.[7]

In either case, corruption is a product of both individual characteristics and social influences, and its welfare impact will incorporate both costs and benefits that may warrant different emphasis in terms of policy preferences. The determinants of corruption, therefore, must be analyzed from the perspectives of individuals and of society. The analytic framework proposed below employs the rational individual choice model that has been widely used in the crime literature, and incorporates three additional structural variables to account for societal influences.

Model for Policy Analysis

Consider the social loss minimization function,

$$W_L = W_L(S, R, f', CP), \qquad (3)$$

which has the same properties as the Becker-Ehrlich model used in the crime literature.[8] *S* is the net social cost which results from corrupt practices; *R* is the direct cost of corruption; *f'* is the social loss per violator convicted; and *CP* is the number of corrupt practices.

The equations of the model are:

$$S = SC(CP) - SB(CP) \qquad (4)$$
$$SC' > 0 \qquad SB' > 0$$
$$SC'' > 0 \qquad SB'' < 0$$

$$R = R(p, CP),\ R = (R_1, R_2) \qquad (5)$$
$$R' > 0$$

$$f' = kf, \qquad k > 0 \qquad (6)$$

$$CP = CP(p, f, Y_i/Y_1, \delta, \pi, \gamma) \qquad (7)$$
$$\frac{\partial CP}{\partial p} < 0, \frac{\partial CP}{\partial f} < 0, \frac{\partial CP}{\partial (Y_i/Y_1)} > 0$$
$$\frac{\partial CP}{\partial \delta} > 0, \frac{\partial CP}{\partial \pi} > 0, \frac{\partial CP}{\partial \gamma} < 0.$$

In equation (4) net social cost (*S*) is stated as the difference between social costs (*SC*) and social benefits (SB). Social costs result from the external diseconomies of corruption; they include economic costs and social damages imposed on third nonconsenting parties, such as the negative implications of violating the rules and norms of society. Social benefits include welfare gains which result from cases 1 or 2, described earlier, and account for the personal gains of the parties who are engaged in corrupt practices. Both SC and SB are related to the level of corrupt practices (CP) and tend to increase as CP increases. Since $SC'' > 0$ and $SB'' < 0$, it is assumed that social costs and benefits are subject to increasing and diminishing returns.

In equation (5) the direct costs of corruption (R), are expected to increase as a result of increases in CP and in the probability of apprehending and convicting the violators (p). Additional resources (R_1) will be used in discovering violations and convicting violators, and at the same time resources (R_2) will be used by the violators to conceal their corrupt practices and to minimize punishment if they are convicted. Equation (6) defines the social cost of punishment (f'). The size of punishment (f), such as the length of imprisonment, is translated into its social cost equivalent f' by a coefficient k indicating the form of punishment imposed on the violator.

Equation (7) relates the level of corruption to p, to f, to the proportion of illegitimate income Y_i to the level of legitimate income Y_1, and to three structural variables: δ indicating the size of bureaucracy; π the degree of ambiguity of the property rights legislation; and γ the degree of political participation in the production of public goods.

The deterrence variables, p and f, and the coefficient k are borrowed from Becker's crime model to explain some aspects of corruption that would be dealt with in a normal criminal activity. As specified in the model, varying p, f, and k is expected to alter the nature of individual optimization. For example, public officials with relatively low pay are likely to react to these variables, and when society raises the values of these variables, they are expected to temper and systematize their practices of corruption.

The structural variables account for society's reaction to corruption under varying arrangements of organizational, legal, and political structures. The organizational variable δ is expected to vary with CP in the same direction. Increased bureaucratic inefficiency inflicts additional time and resource costs on the private sector, and this, in turn, presses potential violators to economize by initiating some form of corruption, such as bribery, to obtain expedited processing. Likewise, π, indicating the degree of vagueness and impotence of property rights legislation, is expected to vary directly with CP. An example would be the difference in interpretation of property rights concerning the ownership of groundwater and air resources. Public officials are more likely to participate in corrupt transactions involving air pollution, which is subject to vague property rights, than in the case of contamination of groundwater, where ownership expectations are vociferously exercised.

The third variable, γ, is the degree of political participation in the production of public goods. It indicates the probability of detecting violations and varies inversely with CP. Low political participation, rational ignorance, or absence of political pressure groups is expected to reduce the probability of detecting corruption. Nepotism and patronage as methods of awarding governmental jobs and contracts are practices that are difficult to maintain under intense public scrutiny. Political participation in elections and the degree of attention that society devotes to political and administrative processes will vary for many reasons. Wars or other major upheavals tend to divert the attention of society, and the expansion of economic activity that results increases the opportunity to engage in corrupt acts. Variation in the business cycle itself would be expected to affect the political participation variable as well. Many activities that might be condoned or ignored during good times become the focus of investigations when conditions worsen and budgets become tighter.[9]

Note that these variables correspond to the structural variables and initiating forces that determined the level of corruption and the resulting changes in society's welfare as described in Figure 1. The policy implications that follow from the deterrent variables, however, are more restrictive than in the case of crime control.[10] As emphasized earlier, corruption results from a collaborative effort between public officials and private parties. The economic impact of corruption relative to crime as defined in criminal law is in the form of "public bad," and the extent of its adversity is likely to depend on the degree of resistance of subjected society. The deterrent variables are likely to be less effective, since corruption is a much more calculated effort, and if corruption is uncovered, often the liability is joint, a factor which may significantly reduce the *ex ante* effectiveness of the deterrent variables.

Thus, the individual rational choice model may be useful for the analysis of corruption that is clearly detrimental. However, when corruption results in both social costs and social benefits, the addition of structural variables pertaining to the resistance level of society is appropriate. In the corruption function introduced above, both public officials and the private parties involved are expected to react to δ, π, and γ. Just as p and f are deterrent variables in the utility functions of violators, δ, π, and γ will be additional incentives or disincentives, significantly determining the level and type of corruption in a society.

Policy Implications of the Model

The model suggests three distinct policy approaches that a society can employ regarding corruption. Maintenance of the status quo is the first policy option, with no additional resources employed to detect or punish violators. The second approach stresses individual-level deterrent considerations as a means of reducing corruption, with additional resources devoted to increasing the probability of detection and punishment of parties engaged in corrupt transactions. The third alternative concentrates on the alteration of the structural conditions relevant to corruption. Additional resources (which will be defined as R_3) would be utilized to clarify property rights, make legislation more congruent with social demands, reduce bureaucratic inefficiency, and increase political participation. In order to maximize social welfare it is necessary to tailor the public policy response to the type of corrupt activity in question.

Recall that corruption was dichotomized as being either beneficial or detrimental, according to its net impact on social welfare. The three public policy options must now be evaluated to determine which choice will maximize social welfare for a given type of corruption. The first option, maintenance of the status quo, can be employed for either type of corruption if certain conditions are present. To justify choosing this option, additional costs required for any change in policy must exceed the benefits derived from reducing corrupt practices. This means that the costs of the corrupt activity must be less for society than the costs associated with any method of reducing the illegitimate practice. Another condition that could justify the status quo would be the society's apparent contentment with the current level of corrupt practices, so that there was no incentive to seek another position. Society in this case can simply ignore the problem.

This option represents an equilibrium position in which the marginal costs of corruption are roughly equivalent to the marginal social benefits attainable from corruption reduction.

The second policy alternative involves increasing p and f. This policy option is desirable only for detrimental corruption, and is justified by the same logic as the deterrent model in the standard crime control literature. Since the net social impact of detrimental corruption is negative, additional resource allocation to improve deterrent considerations is appropriate until the point at which $W_L(S,R,f)$ is minimized.[11] Notice that increasing the deterrent considerations is inappropriate for beneficial corruption, because the net social impact of the activity is positive to begin with, and any additional R_1 and R_2 would detract from the marginal social welfare.

The third policy option requires some combination of the following measures: improving bureaucratic effectiveness, legal changes clarifying property rights or increasing the congruence between social demands and political outcomes, and efforts aimed at increasing political participation. These structural alterations will involve resource utilization R_3. To justify this option resource costs associated with corrupt practices (SC,R,f') must exceed R_3. Symbolically, this means

$$dW_L(S,R,f') > dW_L(R_3). \qquad (8)$$

Note that increased structural efficiency will also yield obvious external benefits to the noncorrupt society $W_G(R_3)$. If these benefits are significant, then

$$[dW_L(S,R,f') + dW_G(R_3)] > dW_L(R_3). \qquad (9)$$

These three options suggest that social welfare is maximized when corruption control policy is directed at the appropriate type of illegitimate activity. Since beneficial corruption is caused by inefficiencies in the structures of society, the appropriate policy response will be to concentrate additional resources on making improvements in social structures. If instead individual deterrent considerations are enlarged to reduce beneficial corruption, then society will experience welfare losses due to increased social costs related to the additional R_1, R_2, and f'.

Conclusions

This research has explored the topic of corruption from the combined perspectives of economics and political science to develop an integrated approach for the development of coherent public policy. To that end it was necessary to link the causes (individual or structural) and consequences (positive or negative) of corruption, and examine the situation in the context of social welfare effects.

One of the theoretical improvements developed in this work is that corruption should not be thought of as a single phenomenon, but as a dichotomy characterized by the net impact of the illegitimate activity on social welfare. If the net impact is positive, corruption is categorized as beneficial; if it is negative, corruption is categorized as detrimental.

From this conception we develop a model in which policy choices related to corruption may be systematically evaluated from a welfare optimization perspective. This model has implications for public policy concerning corruption, and suggests that in cases of beneficial corruption the appropriate strategy may warrant additional resources to alter inefficient social structures. This is a significant departure from the traditional approach to administrative corruption, which treats all illegitimate transactions as though they were the same as most other criminal activity, and thus relies upon individual deterrent policies.

The model accounts for variations in the distributional impact of corruption as well. However, the choices of welfare optimization strategies must ultimately be tempered by a society's notions of distributive justice and fairness. This research has provided a systematic framework for evaluation of public policy choices, and a basis from which the distributive questions can be approached. Further research is required before any definitive statements can be generated concerning distributive equity and corruption control policies. It is our hope that this research will stimulate further analysis that will answer some of the remaining questions.

Notes

1. The Pareto optimum setting is an output performance criterion in the neoclassical interpretation of economic efficiency. In this analysis it is used as a reference. Other constructs of normative output performance can also be used. For more on Pareto optimality and other forms of outcome performance criteria, see De Allesi (1983).

2. In this analysis we assume a society where government involvement in the economy revolves around public goods production. Individuals are rational, and through a market-oriented collective decision-making mechanism they reveal their preferences. Again, these assumptions are made to reveal the essential features of corruption in a market-oriented society. It is important to emphasize that a similar conception of corruption can be also used in centralized or authoritarian societies.

3. Every point on the welfare frontier $U_A U_B$ is Pareto efficient, and represents a possible distribution of real income (utility) between two income segments *A* and *B*. The Bergson-Samuelson social welfare function (*W*) is one of the most commonly used analytic devices, representing society's distributional judgment. In this study, *W* is used in its general form, *W*( ), to generate arguments on distributional consequences of corruption. Its point of tangency with the welfare frontier $U_A U_B$ determines the most desirable allocation among all possible Pareto-efficient allocations. For more on this see Tresch (1981, pp. 17–42).

4. Note that the generalized Paretian efficiency criteria extend to all types of individual optimizations and include additional constraints relating to the system of property rights, including transaction and adjustment costs. See De Allesi (1983).

5. From the economist's perspective, this implies that additional costs must be incurred to introduce structural changes. Usually these costs are substantial because of the inefficiencies involved in collective decision making. Implementation of a cost-effective measure may be delayed due to political considerations.

6. When welfare maximization is divided into separate efficiency and distributional conditions, it can clearly be shown that *F* is Pareto optimal, and even though it is justifiable by society's welfare function, it is not a welfare maximum *P'*. In this context, corruption which moves society from *P* to *F* is beneficial. Compensation schemes such as nondistortionary taxes or subsidies can be introduced to move society to point *P'*. The transition toward welfare maximum can also be eased by clarifying and redefining property rights and by improving the political structure.

7. The distributive consequences of egalitarian, Rawlsian, and utilitarian formulations can be clarified on a generalized welfare frontier which consists of both upward- and downward-sloping sections. On a generalized $U_A U_B$ diagram, then, an egalitarian would select a point on a 45° line. A Rawlsian approach will favor distributional outcomes when a minimax point is reached on the welfare frontier, and the utilitarian will seek the tangency point on the downward-sloping portion of the welfare frontier. In this study, we use a Pareto welfare frontier which has a downward slope, as shown in Figure 1, with no intention of reasoning in favor of any of the three outcomes. Following the convention, we refer to the Pareto welfare frontier because of its theoretical convenience in dichotomizing efficiency and distributional problems. For more on this see Graaff (1975, pp. 59–70).

8. Without specifically referring to corruption, Becker (1968) and Ehrlich (1973, 1975) stress two cost variables: one associated with the probability of apprehension and conviction *p*, and the other with the extent of punishment *f*. By establishing a causal link between the crime rate and each of the two variables, Becker and Ehrlich argue in favor of the deterrent method of preventing crime.

9. Corruption is assumed to exist in both liberal and conservative administrations. What might be expected to vary is the location of the corrupt transaction. Liberal administrations tend to increase expenditures in the area of social programs, which would provide increased accessibility to corruption for traditional liberal constituencies such as the poor and labor. Conversely, conservative administrations tend to increase appropriations for business and military subsidies, and corruption in these areas would be expected to favor traditional conservative constituencies.

10. There are several empirical investigations in the literature that suggest the possibility of reduced participation in specific cases of crime as either the probability of apprehension and conviction *p* or the degree of punishment *f* increased. For a review of these studies see Tullock (1980), and Witte (1980).

For studies which challenge the deterrent method see Bowers and Pierce (1975), Cook (1980), Forst (1976), Passell and Taylor (1977).

11. See Becker (1968).

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HEGEL'S CRITIQUE OF LIBERALISM

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A recent and perhaps surprising development in political theory has been the revival of neo-Kantian liberalism, with its doctrines of the neutral state and equal concern and respect. Critics of liberalism have suggested, however, that Kantian notions of rights and rationality are too thin a foundation on which to build satisfactory forms of community and political life. In this paper I examine the critique of rights-based liberalism by returning to the philosophy of Hegel. Hegel's position, I suggest, provides us with a much needed middle ground between liberalism and its contemporary critics. Like the modern communitarians he is critical of the individualistic and ahistorical conceptions of rights underlying the liberal polity, but like many liberals he is skeptical of the claims to recreate a democratic, participatory Gemeinschaft *that would leave citizens defenseless before their particular communities. I conclude that like Montesquieu before him and Tocqueville after, Hegel looked to the "corporations" or intermediary associations to skirt the extremes of the market place and civic virtue.*

The last decade or so has witnessed the revival of a kind of moral and political theorizing that owes an explicit debt to Kant.[1] The core strategy sparking this neo-Kantian revival has been an attempt to restore the political philosophy of liberalism on a rationalized and purified foundation. Clearly, the most powerful example of this purified liberalism has been John Rawls's *A Theory of Justice* (1971). Basing his arguments on Kant's distinction between justice and utility and on his conception of the "hypothetical contract," Rawls and others have attempted to "construct" a framework for choosing between moral principles that assumes nothing more than the existence of rational, self-legislating agents. By asking us to abstract from all contingently acquired and hence empirical characteristics about ourselves, the aim of these theorists has been to derive a set of legal and political institutions that accord equal "concern and respect" to everyone's interests. Indeed, so successful has this strategy appeared that one of its proponents has called it "a new liberal paradigm" (Fishkin, 1984, p. 755).

This "new liberal paradigm" has characteristically adopted procedures and assumptions from a variety of very non-Kantian sources, especially neoclassical economic theory. There are at least four presuppositions concerning the nature of rationality and action that underlie these sources.[2] First, rationality is, on this account, exclusively a predicate of individual actors and actions; this is the principle of methodological individualism. Second, rationality is concerned with

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

means and not ends. It is a form of calculation that allows the agent to acquire the objects of his desire, rather than prescribing what kind of objects he ought to desire; this is the principle of value-neutrality. Third, the natural goal of each agent is to maximize the number of pleasures and minimize the number of pains; this is the principle of psychological hedonism. And fourth, the problem of politics is conceived as finding ways of limiting the infinity of human desires so that each can coexist with all under the rule of universal law. This is the principle of the social contract or the "natural condition," which is in turn said to generate the conditions for political legitimacy. Out of these purely hypothetical assumptions about human nature, contemporary political theorists have proceeded to elaborate rules of justice which claim to be binding on all rational beings everywhere and always.

And yet the revival of neo-Kantian liberalism has not gone unchallenged. The intellectual pendulum has now begun to swing the other way. The arguments expouded by thinkers as different as Michael Walzer (1983), Alasdair MacIntyre (1981), and Michael Sandel (1982) claim that the solitary, individualistic, and "unencumbered" self of Kantian liberalism is too thin a basis from which to generate political culture or community. In the first place, they deny the tenet of methodological individualism by arguing that rationality may be predicated not merely of individual actions, but of the institutions and even the political culture that make these actions possible. Second, rationality is valued not simply for its ability to achieve ends, but is an end in itself, something valued for its own sake. Third, the comprehensive good for human beings cannot be regarded as the satisfaction of private pleasures, but is intimately bound up with the achievement of the good of others. We are what we are by virtue of our membership in a community of shared meanings and values. And finally, the political community is not on this account merely an aggregate of atomistic individuals peering through a "veil of ignorance," but is at least partially constitutive of what it is to be a human being. Political life is not just a burden to be borne or a cost to be calculated, but satisfies the human need to belong to a community of rational beings within which one's own moral and intellectual powers may develop. As one of the proponents of the new communitarianism has put it: "To say that the members of a society are bound by a sense of community is not simply to say that a great many of them profess communitarian sentiments and pursue communitarian aims, but rather that they conceive their identity . . . as defined to some extent by the community of which they are a part" (Sandel, 1982, p. 147).

It seems obvious to me that neither of these views is wholly correct. The first attempts to discover rules of social life that would be binding on all rational agents, but its own prescriptions are often so general and abstract as to be void of moral content. Only by abstracting from everything we already know about ourselves, our lives, and our histories will we be in a position to provide a solid and unimpeachable ground for choosing moral principles. I would argue, however, that the result of this operation of abstraction is not to provide a stronger foundation from which the self can choose, but no foundation at all, since there is literally nothing left to the self.

If the neo-Kantian paradigm suffers, then, from excessive abstractness, the new community or participatory model suffers from the opposite problem. In its assumption that everything we are we owe to the particular communities of which we are a part, the communitarians appear dangerously indifferent to the quite legitimate claims that individuals can make against their communities. All we are told is that particular communities are the outcome of our communal or political natures, which leaves the individual no room to criticize those communities except in their own terms. The result, often the opposite of what is intended, is merely to re-endorse those patterns of social and political life without asking whether they are rational or whether they ought to exist. These "re-endorsement theories," as Ernest Gellner has called them, tend uncritically to accept whatever forms of life or culture happen to exist. "Forms of life and cultures," he says, "are precisely what thought does not and need not automatically accept. Cultures must not be judges in their own case" (Gellner, 1974, p. 20).

It is not my purpose in this paper to attempt to pass judgment on this debate. Instead I would like to examine the genesis of the critique of rights-based liberalism in the philosophy of Hegel.[3] Indeed, many of the criticisms that are made of liberalism today—its abstract, individualistic methodology, its excessive concern with private nonpolitical goals, its insensitivity to public issues of citizenship and civic virtue—were all decisively brought to bear near the beginning of the previous century by Hegel. In a sense the whole debate between contemporary liberals and their critics has something of the character of a reinvention of the wheel. It is my contention here that present-day critics of the culture of rights-based liberalism would do well to return to Hegel as the theoretical source of at least some of their beliefs (Plant, 1973; Taylor, 1979).

To cash this thesis I will have to show first what Hegel thought was defective with the tradition of rights-based liberalism as this was handed down from Hobbes and Locke to Rousseau and Kant. After outlining Hegel's methodological objections to this tradition, I will turn to what he takes to be the faulty political practices that it engenders. Only then will I examine Hegel's positive conception of a community or political culture based upon the notion of *Sittlichkeit*, or the concrete ethical life of a people.

Hegel's position, I want to argue, provides us with a *via media* between the two alternatives outlined above. Like modern communitarians, he is critical of the individualistic and ahistorical concepts of rights underlying the liberal polity, but like many liberals in both his day and ours, he rejects any attempt to return to some form of democratic participatory *Gemeinschaft* based upon immediate face-to-face relations. Instead of rejecting liberalism out of hand, Hegel accepts many of the institutions of the modern world, especially its distinction between civil and political life, as uniquely capable of ensuring the citizen's rational devotion and informed consent. It is this distinction, I want to suggest, that provides Hegel's most lasting and defensible contribution to political philosophy.

The Critique of Natural Rights

Hegel's critique of modern natural rights theories is developed most powerfully in a book-length essay published almost twenty years prior to the *Philosophy of Right*, and called *On the Scientific Ways of Treating Natural Law*. Here he takes to task what he takes to be the two most important methodological approaches to the study of rights. The first he identifies with the "empirical" approach, by which he means the naturalistic theories of Hobbes and Locke (Hegel, *NL*, 1975, pp. 59–70). These thinkers are called empiricists because they sought to derive a theory of rights from certain purported facts or needs that human beings have. These needs, which were thought to be prior to the emergence of civil society, and therefore the real or genuine needs of man, could then be used as the primary building blocks out of which human rights could be constructed. Thus the Hobbesian-Lockean account of rights derives from the root human need for self-preservation, or in Montesquieu's more expansive formulation, from "that tranquility of mind which arises from an individual's opinion of his security" (Montesquieu, 1949, bk, XI, sec. 6, p. 151).

Still, the empirical approach to rights has its origins in the work of Thomas Hobbes, whom Hegel regarded as the "revolutionary" founder of the whole modern tradition (*LHP*, 1955, vol. 3, pp. 92–3). The novel and shocking character of Hobbes's insights consisted in the fact that he was the first to derive a teaching about political obligation from a doctrine of the will alone. The state for Hobbes is no longer conceived as it was by Plato and Aristotle, as a duplication of principles already "out there" in nature that can be discovered by dint of human reason. Rather, for Hobbes, the state is the result of an active choice by which we impose our wills upon nature. This emphasis on the active role of choice or will is what distinguishes ancient from modern conceptions of right (Hegel, *LHP*, 1955, vol. 3, pp. 114–15). Indeed, nature is no longer able to provide a ground for political obligation precisely because the idea of nature has undergone a radical change. Nature is no longer regarded as beneficiently organized for human ends and purposes. Rather, it is seen as something indifferent or even hostile to human life, devoid of what Nietzsche would call "metaphysical comfort," and therefore something to be "conquered." It is necessary, then, to "construct" social reality out of the raw materials provided by nature, "to the end [of securing] as far as matter, and human force permit, such ends as human life requireth" (Hobbes, 1977, p. 478).

The upshot of this new activist or constructivist conception of the will is threefold. First, it blurs the ancient distinction between doing and making, *praxis* and *poiesis*. Politics is no longer regarded as an activity the point of which is the activity itself performed well. It becomes more like a technique in the modern sense of social engineering, which aims at the skillful production of a legal order whose aims are the protection of health, safety, and comfort (Habermas, 1974, p. 42). Second, Hobbes regards politics as capable of achieving scientific or epistemic status precisely because, and to the extent that, its subject matter has been made by us. The idea here is that nature provides no intrinsic standard of its intelligibility. Accordingly, what knowledge of either the social or natural worlds we possess is the product of a free construction whereby man, the knowing subject, imposes an order and design of his own making. Because, for Hobbes, there is no presupposition of a preestablished harmony between our knowledge and the world, the only test of the adequacy of our ideas is "operational"—whether they "work" (cf. Strauss, 1953, pp. 173–74). Third, Hobbes tried to demonstrate that only a society that has been artificially created along the lines of the modern science of nature is capable of imposing obligations upon its subjects. If, then, society is purely an artificial construct, the question for politics becomes not the ancient one—"what is the best life for man?" or "how ought I to live?"—but "why should I obey the law?" Questions of obligation, not virtue, have become primary in the new political science (Salkever, 1974, pp. 78–92).

Even while Hegel credits Hobbes with these epoch-making insights, he still claims to have detected a crucial ambiguity in the empirical approach to rights. Empiricism claims to discover the most universal features of human beings as such by means of a kind of thought-experiment, hypothetically stripping or peeling away everything we have acquired through the influence of history, custom, and tradition. But if empiricism is to remain faithful to its own epistemological premises, it cannot, strictly speaking, distinguish the natural from the conventional. In Hegel's terminology, empiricism is incapable of separating the "necessary" from the "contingent" without introducing extraneous assumptions: "Empiricism lacks in the first place all criteria for drawing the boundary between the accidental and the necessary; i.e., for determining what in the chaos of the state of nature or in the abstraction of man must remain and what must be discarded" (*NL*, 1975, p. 64). The problem is that rather than merely collecting data or counting facts, the empiricist is led to abstract some singular aspect of human experience, such as the drive for self-preservation, and to elevate it to the status of a first cause capable of accounting for society. Explanations of this sort, Hegel shows, tend to be vacuous, since they do no more than read back into nature what they have already taken out. Thus despite Hobbes's bold claims to have discovered the true ground of human rights in the most elementary needs of men, in the end he does no more than accept our empirically given needs as binding.

The more promising strand of natural rights theory, then, would seem to be represented by the "formalist" approach of Kant and Fichte, who were well aware of the difficulties with empiricism (Hegel, *NL*, 1975, pp. 70 ff.). Indeed, their formalism is no more than a deepening and a radicalization of the critique already levelled against Hobbes and Locke by Rousseau (1964, pp. 213–20). In the *Philosophy of Right* Hegel goes out of his way to commend Rousseau for "adducing the will as the principle of the state" and not "a principle, like gregarious instinct, for instance, or divine authority" (*PR*, 1972, par. 258R, pp. 156–57).

This remark is clearly an allusion to Rousseau's emphasis on the will in the *Social Contract* but perhaps even more importantly to the philosophical anthropology of the *Discourse on the Origins of Inequality* in which he argues that freedom from natural causality is the distinctive mark of the human. Hobbes and Locke, Rousseau argued there, were correct to refer the standard of right back to nature alone. What they failed to clarify, however, is that nature provides at best only a kind of negative orientation from which we must break if we are to understand the true ground of rights. The source of right must be sought in that quality that escapes or transcends nature. This quality was described by him as the power of the will or free agency, one of the "essential gifts of nature" precisely because it lacks empirical direction or orientation (Rousseau, 1964, pp. 113–14, 168). This is the point that Hobbes had only imperfectly understood. While he correctly saw that the will is the basis for all obligation, he failed to distinguish radically enough the will from nature. Consequently, when he came to describe formally the function of the will, he simply turned it into "the last appetite in deliberation" (Hobbes, 1977, p. 54). But if the will is an "appetite," even if only the "last" one, it still remains bound by the laws of natural causality and fails to achieve the freedom that is its essence.

Even as Rousseau attempted to divorce the will from all naturalistic or teleological directives, it was Kant alone who at last succeeded in articulating a pure "formalism" of the will. By "formal" I mean here that Kant was more interested in the form or structure in which moral imperatives are framed than in their content or direction. This formalism led to an almost exclusive emphasis on the "inner" side of action, the purity of the motive or the intent, as opposed to the "outer" side, the accomplished result. Goodness, then, applies only to good intentions or the good will, which is the one thing that Kant calls "unconditionally" good (Kant, 1964, p. 9). Of course, the paradoxical conclusion of Kant's alleged moral formalism is that while he attributes absolute or unconditional goodness to the will alone, he is led to deny it to the external world of action and society, both of which are conceived as governed by causal, natural laws, and as such indifferent to moral praise and blame.

Kant's rigorous separation of the will from its actions is not as bizarre as it might at first appear. If we regard human action from the empirical or "phenomenal" side, we see that it is determined by a variety of heteronomous ends and purposes. Seen in this light, our motives and purposes are as much a part of the determined order of nature as are any other physical occurrences. And yet, as Kant tries to show in the famous "Third Antinomy" of the *Critique of Pure Reason* (1965, pp. 409–17) the all-embracing laws of physical causality cannot drive out completely the dimension of human freedom. Kant believes that if we abstract from all the diverse ends and purposes for which we strive, we will find underlying them all the capacity for willing and choosing, the capacity to be self-directing or self-determining. Kant refers to this capacity as the power of "pure practical reason," which alone is sufficient to distinguish us from the laws of physical causation. We alone possess a form of "moral causality," or the capacity for "spontaneity," which escapes nature. Thus our unique status as moral beings derives from the fact that we have the capacity not merely to live in various ways, but to make the laws by which we choose to live. It is this power of initiation, rather than any determinate end we might pursue, that testifies to the inherent "dignity and sublimity" of man and forms the only ground of right (Kant, 1964, p. 107).

The only legitimate limitation Kant can imagine on the will's capacity for self-direction is a purely negative or formal one. While the will, considered as a mere capacity for self-legislation, provides no object or action that we can affirm or deny, it does at least provide a test for determining whether the action to which it points is a moral one. This is the principle of universalizability. According to this principle, the maxim informing our actions must be framed in such a way that it would obligate everyone who found himself similarly circumstanced. Thus the first and most famous formulation of the categorical imperative reads: "Act only on that maxim through which you can at the same time will that it should become a universal law" (Kant, 1964, p. 88). Or again: "Act as if the maxim of your action were to become through your will a universal law of nature" (Kant, 1964, p. 89). Moral action, then, must be characterized by the same abstract generality or universality as the laws governing the physical world. A law that cannot be consistent, that is, universally willed to apply to everyone who finds himself similarly circumstanced, cannot have moral or categorical validity. At best such maxims would be like Hobbes's councils of prudence—to be followed if they serve the agent's interests—rather than moral laws which obligate because of their very form (Kant, 1964, p. 86).

We need not continue with this account of Kant's moral formalism. More to the point are the difficulties to which this conception of morality gives rise. Specifically, by concerning himself almost exclusively with the form of moral judgment, that is, with how something is willed, Kant leaves himself open to the charge of indifference to what is willed. To what objects or set of objects is the moral law supposed to apply? According to Hegel, Kant can give no answer to this question, because the moment he attempts to specify some content for the law, he immediately violates its universality:

> For practical reason is the complete abstraction from all content of the will; to introduce a content is to establish a heteronomy of choice. But

> what is precisely of interest is to know what right and duty are. We ask for the content of the moral law, and this content alone concerns us. But the essence of pure will and pure practical reason is to be abstracted from all content. Thus it is a self-contradiction to seek in this absolute practical reason a moral legislation which we would have to have a content, since the essence of this reason is to have none (*NL*, 1975, p. 76).

Hegel's charge here—that Kantian morality is an "empty formalism" that can generate no "immanent doctrine of duties," and that it is therefore reduced to "preaching" an ethic of "duty for duty's sake" (*PR*, 1972, par. 135R, p. 90)—has been more or less accepted by generations of interpreters. To illustrate his point, Hegel refers explicitly to an example that Kant himself used on a number of occasions to illustrate the categorical imperative (*NL*, 1975, p. 77; cf. Kant, 1956, pp. 26-27; 1977, pp. 70-71). The example is that of an agent who has been entrusted with a sum of money, the owner of which has died and has left no evidence of its whereabouts. In one of his formulations of this example Kant even asks us to imagine that through no fault of his own the trustee has suffered complete financial disaster, while the heirs of the deceased are "rich, uncharitable, thoroughly extravagant, and luxurious, so that it would make little difference if the aforesaid addition to their property were thrown into the sea." Even under these conditions, Kant claims, the trustee would be wrong to keep the deposit, for reasons that would be plain to "a child of around eight or nine years old." To universalize the maxim of that action, that it is right to embezzle deposits whenever it is in my interests to do so or whenever I know that I will not be caught, could not be universalized without contradiction; for to universalize this maxim would be to destroy the conditions of trust and confidence that make not only deposits but any safety and security among men possible at all. Such actions are not only self-contradictory but self-defeating, since they threaten any viable test of rights.

Hegel's answer to this application of the moral law is well known, but still deserves to be cited in full:

> But where is the contradiction if there were no deposits? The non-existence of deposits would contradict other specific things, just as the possibility of deposits fits together with other necessary specific things and thereby will itself be necessary. But other ends and material grounds are not to be invoked; it is the immediate form of the concept which is to settle the rightness of adopting either one specific matter or the other. For the form, however, one of the opposed specifics is just as valid as the other; each can be conceived as a quality, and this conception can be expressed as a law (*NL*, 1975, p. 77).

The point of this passage is that Kant's universalization principle cannot establish what he would like it to. Universalization can ensure only consistency or rationality of action irrespective of social context or utility. Thus, to say that the embezzlement of deposits is wrong assumes that if such a practice were generalized it would result in a world where the continued existence of deposits, and hence property, would be thrown into question. Hegel's point, however, is that a world without property could not, even on Kant's account, be regarded as morally inferior to the existing world. It would only be a different world from the one in which we now live. There is only a "contradiction" if we expected others to respect our property and return deposits, but had no intention of doing so to theirs. Thus Kant's universalization principle may be an effective means of unmasking the moral hypocrite, but would be useless against the consistent evil doer. It might be useful in dealing with the Willy Suttons of the world, but would be silenced in the face of the Marquis de Sade.

Hegel's point seems to me to be twofold. First, because it operates at such a high altitude, the categorical imperative is incapable of demonstrating whether certain social institutions are valid or invalid. As the example above illustrates, Kant can at best show what kinds of actions are ruled out if certain kinds of institutions are presupposed. In Hegel's view this testifies to a deeply rooted conservatism in Kant's thought, since by a kind of "sleight of hand" (Lukács, 1975, p. 160) he leaves the social and political world simply as it is.

Second, Kant's preference for sheer consistency, as opposed to moderation or compromise, seems to mask a moral zealotry where the most extreme policies are advocated so long as they are sincerely intended. Hence, in the appendix to *Perpetual Peace* (in *Political Writings*, 1977), Kant derides the "political moralists" who debase morality by making it serve partisan interests. Instead he wants politics to serve morality. The "moral politician" is one who upholds rights as the "limiting condition" of what is politically permissible. The result is to moralize political relations by insisting that "a true system of politics cannot therefore take a single step without first paying tribute to morality" (Kant, 1977, pp. 117-18, 125). What Hegel fears is that this elevation of morality as the judge of politics has liberated a kind of extremism that rules out policies based upon the prudential reconciliation of interests. As with Max Weber later on, the implication of what Kant says is that politics is best when commitments are at their highest. But, and this is crucial, there is no means of distinguishing between commitments except on the purely formal grounds of logical consistency (cf. Weber, 1949, pp. 10, 57).

In the final analysis, then, Hegel believes that the problem with Kant's formalist approach to rights derives from his attempt to identify some area of human freedom apart from the determined order of nature. Since freedom cannot be found within nature, it had to be located over and against it in the will, the sheer capacity for spontaneous action. The difficulty here is that by confining freedom to the activity of willing alone, we deny that it can be found in the external world, since there we see no unconditioned wills but only actions which form a series of links in an infinite chain of causes. Kant's constriction of freedom to the noumenal realm, the sphere of pure intentions, has the effect of rendering it altogether nugatory for practical purposes. Nietzsche was perhaps only the most ruthless in exposing the illusions of this purely Kantian or German "inwardness." The danger, he wrote, is that "the internal substance [the good will] cannot be seen from the outside, and so may one day take the opportunity of vanishing, and no one will notice its absence any more than its presence before" (Nietzsche, 1957, p. 26).

Rousseau and the French Revolution

Hegel's critique of modern natural rights philosophy is not merely methodological, but substantive. He regards it as revolutionary in its essence and responsible for the excesses of the French Revolution and the terror in particular. And yet while Hegel, like Kant before him, was chastened by the actual experience of revolution, he continued to celebrate, almost to the end of his life, the principle which he believed the revolution had vindicated.[4] This principle, stated forcefully in his lectures on the *Philosophy of History*, is the right of reason or philosophy to restructure the world. In accord with the general outlook of German idealism, Hegel interprets the revolution as carrying out in practice what Enlightenment philosophers had already vindicated in theory:

> Anaxagoras had been the first to say that *nous* governs the World; but not until now had man advanced to the recognition of the principle that Thought ought to govern spiritual reality. This was accordingly a glorious mental down. All thinking beings shared in the jubilation of this

> epoch. Emotions of a lofty character stirred men's minds at that time; a spiritual enthusiasm thrilled through the world, as if the reconciliation between the Divine and the Secular was now first accomplished (*PH,* 1956, p. 447).

The French Revolution, then, is the final proof and legitimation of the principles of the natural rights philosophers advanced for over a century before. The core of their teaching was the belief that men should be free to make and remake their institutions and their world in keeping with their own critical rationality. Reason alone could liberate men from the inherited weight of custom, tradition, and history, and allow them for the first time to master the world by submitting it to their own conscious designs and purposes. Indeed, Kant only expressed the deepest longings of Enlightenment when he described its philosophy as "man's emergence from his self-incurred immaturity. . . . The motto of enlightenment is therefore: *Sapere aude!* Have courage to use your *own* understanding!" (1977, p. 54).

But even as Kant regarded his own "critical idealism" as carrying out a "Copernican revolution" in both knowledge and morals, he was reluctant to extend this into a doctrine of political revolt. In the *Contest of the Faculties* (in *Political Writings,* 1977), for instance, Kant looked hopefully to the revolution as proof that political action could be guided by moral reason rather than self-interest alone. Even while the revolution, he says, may be filled with "misery and atrocity," such that "no right-thinking man would ever decide to make the same experiment again," it still cannot help but inspire in all "disinterested spectators" a belief in a "moral tendency within the human race" (Kant, 1977, p. 182; translation modified). Yet even if the revolution revealed the openness of politics to moral factors, he regarded the regicide as an ethical sin even worse than murder. In the *Rechtslehre* he remarks that the act of regicide "must arouse dread in any soul imbued with ideas of human right." This feeling of dread is not "aesthetic," which derives from pity for the executed man, but moral in the sense of a "complete reversal of all concepts of right" (Kant, 1977, p. 145). Thus, to the extent that political reform was possible, Kant preferred to begin from the top down. Only existing sovereigns open to moral appeal are valid agents of reform.

It is not surprising that Hegel and the generation that came of age during the French Revolution found Kant's halfhearted application of his philosophy to politics inadequate. For Hegel and his contemporaries like Schelling and Hölderlin, the revolution promised the recreation of a cohesive republican community founded upon Kantian morality. Kantian moral autonomy was regarded as the appropriate support for the cult of ancient virtue and active citizenship so prominent during the 1790s. As early as 1795 Hegel had declared himself a devotee of Kantianism because it vindicated the "rights of man" (*BR,* 1952, vol. 1, pp. 23–24). With the popularization of the idea of right, Hegel wrote to Schelling, it would only be a matter of time until the public began to demand treatment with dignity and respect. Awakened from their slumber, men would begin to think not in terms of "what is" but in terms of "what ought to be." On the basis of his early Kantianism, Hegel went so far as to predict: "From the Kantian system and its completion, then, I expect a revolution in Germany." Thus Hegel hoped to translate into political terms the "kingdom of ends" of the *Foundations of the Metaphysics of Morals* and the "ethical commonwealth" of *Religion Within the Limits of Reason Alone.*

Even while Hegel continued to regard the French Revolution (along with the Protestant Reformation) as one of the great watershed moments of modern history, he later came to see it more as a great moral and political tragedy. The tragic and even nihilistic character of the revolution stemmed ultimately from the philosophical subjectivism that is its source. This is examined most comprehensively in a chapter from the *Phenomenology of Mind* entitled "Absolute Freedom and Terror," where Hegel shows how the revolution originated from an effort to overcome the division or diremption (*Entfremdung*) of culture, under the old regime (*PHM*, 1966, pp. 599–610). The old regime, Hegel argues, is a culture in alienation or estrangement. By estrangement Hegel means a discrepancy between seeming and being, between what a culture says and thinks about itself and what it in actuality is. Under the old regime this discrepancy between seeming and being became especially acute as the nobility's love of honor and prestige, which had earlier defined the ethical ideals of feudalism, became transformed into the spirit of the courtier, the flatterer, and the valet. In Hegel's memorable phrase the "heroism of service" was replaced by the "heroism of flattery" (*PHM*, 1966, p. 527).

Hegel's account of the old regime is clearly indebted to Montesquieu, who saw the sentiments of honor and service as the essential "principle" of feudalism.[5] But he goes beyond Montesquieu in showing how this "noble consciousness" is not a kind of frozen ideal type, but is dialectically transformed by the power of wealth and money into relinquishing its position and accepting a life of comfort and enjoyment. The proud aristocrat, this once "haughty vassal" who sought glory and nobility for himself in battle, has become a property-owning bourgeois seeking the accumulation of goods and property to insulate him from the consciousness of death. This contradiction between the bourgeois consciousness defined by its fear of death and the older aristocratic or warrior consciousness defined by its contempt for wealth and comfort marks the beginning of the end of the old regime.

The final stage of the denoument is revealed most powerfully in Hegel's analysis of Diderot's *Le neveu de Rameau*, the text which lays bare and reveals this contradiction in all its duplicity (*PHM*, 1966, pp. 543–48). This work is virtually the prototype of the prerevolutionary mind torn between the conflicting values of the aristocracy and the bourgeoisie. What emerges from this "disintegrated" consciousness is, of course, the demand for "absolute freedom" from all previous restrictions and cultural restraints. What Hegel describes here is a kind of freedom without context, a nihilistic yearning for oblivion or a "freedom of the void" that is often associated with the modern desire for total emancipation (cf. *PR*, 1972, par. 5R, p. 22). In an earlier section of the *Phenomenology of Mind*, Hegel describes how this modern yearning for radical freedom has emerged from the process of cultural disintegration that preceded it:

> It is here as in the case of the birth of a child; after a long period of nutrition in silence, the continuity of the gradual growth in size, of quantitative change, is suddenly cut short by the first breath drawn—there is a break in the process, a qualitative change—and the child is born. In like manner the spirit of the time, growing slowly and quietly ripe for the new form it is to assume, disintegrates one fragment after another of the structure of its previous world. That it is tottering to its fall is indicated only by symptoms here and there. Frivolity and again ennui, which are spreading in the established order of things, the undefined foreboding of something unknown—all these betoken that there is something else approaching. This gradual crumbling to pieces, which did not alter the general look and aspect of the whole, is interrupted by the sunrise, which, in a flash and at a single stroke, brings to view the form and structure of the new world (*PHM*, 1966, p. 75; see also *ETW*, 1971, p. 152).

The French Revolution looked to Hegel like the attempt to recreate the conditions for social and political harmony that not only the old regime, but all post-classical culture had torn asunder. The revolutionaries, acting out of a desire to bring the doctrines of the philosophers down to earth, directed themselves to removing all traces of transcendence and otherworldliness (Strauss, 1953, p. 15). To bring this reconciliation about the radicals sought to recreate the kind of consensus and public spiritedness evinced by the ancient *polis*. The *polis* experience was based on a devotion to "public goods" at the expense of private interests, and elevated the virtue of the citizen over and above those of the private man or bourgeois. But since these conditions are no longer a part of the modern world, they had to be recreated artificially, that is, on the basis of violence or terror, through a radical, wholesale negation of what is.

Hegel's verdict on this attempt to realize the general will here and now is clear and decisive: It resulted in a "rage and fury of destruction" that had "no inner significance or filling" any more than "cutting off a head of cabbage or swallowing a draught of water" (*PHM*, 1966, pp. 604–05). The claim that we should will to be in all things, or that the general will should be the only legitimate ground of rights, would not only abolish all existing institutions and hierarchies, but would regard "all differences in talent and authority as being superseded" (*PHM*, 1966, p. 601). Nothing would be allowed to exist that is not a product of the general will. Thus even God, the "empty *être suprême*," is said to hover there "merely as an exhaltation of stale gas" (*PHM*, 1966, p. 602). The culmination of the revolution, then, is "the sheer horror of the negative" in which all the "determinate elements disappear with the disaster and ruin that overtake the self in the state of absolute freedom" (*PHM*, 1966, p. 608).

The argument being made here is that while the general will can abolish, it cannot create. It can destroy the old regime, but cannot build a new one. The idea of the general will is that I am only free when I obey the laws that I have myself created. But since the general will is the outcome of a collective decision, it cannot be decided by me alone. If everyone is to be free, then everyone must at least participate in the decision making. There is no sense here as, say, in Hobbes, of authorizing someone else to do the work for me (Hobbes, 1977, pp. 126–28). Freedom is attained for everyone when there is a complete unanimity of wills. But, as Hegel points out, the search for unanimity is a chimera less likely to result in community than in complete anarchy. Since each will necessarily regards itself as absolute, as the supreme expression of the *volonté générale*, there is no standard short of universal agreement that can guarantee the legitimacy of the outcome. Any halfway measure, such as representative institutions or rule by the majority, would be a violation of my inalienable right to self-legislation, to obey laws of my own making. The result is to create a permanent and implacable opposition between the people and their government, which will always appear to them as a particular corporate will, a "faction" interposing itself between them and their true interests (*PHM*, 1966, pp. 605–06).

Hegel demonstrates how this inability of the will to generate a stable political community is related to its inner principle of subjectivity. Since the will has been shorn of all its natural or empirical ends, the only guarantee of its authenticity lies in its inner purity or self-certainty. Sincerity thus becomes the essence of virtue. But herein lies the problem, for if purity of intent is the sole criterion of the worth of action, then men must be judged not according to the outcome of their deeds, but by their subjective convictions or the "law of the heart" (*PHM*, 1966, pp. 391–400). By elevating the purity of one's convictions as the standard of political right, the politics of the revolution became a relentless search to unmask those hypocrites who pursue their own private ends under the guise of public virtue. Just as sincerity was the greatest virtue, so did hypocrisy become the greatest vice. Indeed, the reign of terror was nothing so much as the working out on the public stage of this obsessive concern with the purity of motives.

One might, of course, wonder why hypocrisy, what Judith Shklar (1984, pp. 45–86) has called a relatively "ordinary vice," would be responsible for such a wave of violence and fanaticism. Hypocrisy is, after all, the only vice that pays homage to virtue. Hannah Arendt (1965, pp. 91–104) has argued at length that this desire to root out hypocrisy stems from the revolution's own "favored simile" of itself as tearing the mask, the persona, off a corrupt French society to expose behind it the uncorrupted "natural man." For a theorist like Arendt, for whom politics is in the literal sense a kind of "play acting" where actors become the roles or legal personae that they assume, this search for the natural or authentic man behind the mask can only spell disaster. The natural man was, of course, an invention of Rousseau's, who in the *Discourse on Inequality* had described him as unspoiled, honest, and self-complete; only through society or history does he become corrupt, calculative, and dependent (Rousseau, 1964, p. 179). The object of Rousseau's scorn was, however, a particular species of civil or historical man that he was the first to identify as the modern bourgeois. The bourgeois was for Rousseau not an owner of the means of production, as this term was later used by Marx, but in the first place the possessor of a kind of self-love (*amour propre*) that leads him constantly to live both in and through the opinions of others. Unlike *l'homme naturel* who is fundamentally self-sufficient and drawn to others only through the instinct of pity or compassion, civil or bourgeois man needs others as the means to the satisfaction of his almost limitless desires. What repels Rousseau is that unlike both the natural man and the classical *citoyen*, who are both directly what they seem to be, the bourgeois is "alienated" in the sense of the term described above: he is not what he seems to be; he is a dissembler, a role player, and worst of all, a hypocrite.

From their attempt to resurrect Rousseau's natural man as the norm and measure by which to judge civil man, the revolutionaries sought to recast society wholesale. Just as Rousseau had seen pity as the source of all morality, so did Robespierre regard virtue as the ability to *identify oneself immediately with* the immense poverty and suffering of the majority of the French *peuple*. Virtue thus became synonymous with the capacity to sympathize with the plight of others, the *malheureux*, or the wretched of the earth. The new "republic of virtue" would then turn its "compassionate zeal" on all those suspected of harboring an impure heart or a lack of identification with the nation as a whole. A heightened moral sensitivity thus unleashed a wave of unprecedented ruthlessness and terror. But out of this distrust of the motives of others, the revolutionaries came to distrust their own motives. Recognizing with Pascal that the heart has "*raisons que la raison ne connait point*," the revolution could only end by devouring its own. As Arendt writes:

> Robespierre's insane lack of trust in others, even in his closest friends, sprang ultimately from his not so insane but quite normal suspicion of himself. Since his very credo forced him to play the "incorruptible" in public every day and to display his virtue, to open his heart as he understood it, at least once a week, how could he be sure that he was not the one thing he probably feared most in his life, a hypocrite? (Arendt, 1965, p. 92).

The failure of the French Revolution, then, to create a moral republic stemmed from its transformation of an essentially private conception of virtue as sincerity into a test of citizenship. The problem with this immense privatization of the public realm is that it provides no means of harmonizing the freedom of the individual will with the freedom of all except through recourse to force and coercion. Since men cannot be assumed to be virtuous, they must be "forced to be free." Rousseau's other solution for establishing the authenticity of the general will by including everyone within the decision-making process is no answer. Leaving aside the practical difficulty of convening even a relatively small city, the participation of each citizen in public deliberation and debate cannot create a community precisely because it already presupposes a community. Rousseau's only answer here was recourse to the semi-mythical figure of the "legislator" who, like Moses, Romulus, and Theseus, is capable of creating a people virtually *ex nihilo* (Rousseau, 1978, bk. II, Ch. 7, pp. 67–70).

Hegel's answer to this problem was to invoke the figure of a modern day Theseus, namely Napoleon, as the founder of the state. In his Jena lectures on the philosophy of mind Hegel remarks that, unlike earlier natural rights theorists, for whom the state has its origins in the rational deliberations of autonomous agents, for him, "all states are founded by the sublime power of great men" (*JR*, 1969, p. 246). As in his earlier study of *The German Constitution*, Hegel is obviously thinking here of Napoleon as the restorer of the state after its dissolution in the acid of revolutionary turmoil (cf. *PW*, 1964, pp. 145, 219, 245). "In this way," he says, "Theseus founded the Athenian state; also in this way during the French Revolution a terrible power held the state generally" (*JR*, 1969, p. 246).

While Napoleon, this "terrible power," is not mentioned by name in either the Jena lectures or the *Phenomenology of Mind*, it is clearly not the man but the principle he embodies that Hegel admires. This is the principle of modern constitutional government based upon the rule of law and the division of society into distinct public and private spheres. But when the basis for this state has been laid, the work of its architect is made redundant. Such was the case with Robespierre, of whom Hegel says that "power abandoned him, because necessity abandoned him and so he was violently overthrown" (*JR*, 1969, p. 248). Thus like the original Theseus, Napoleon, the modern tyrant, is fated to disappear from the scene which he has helped to create. Hegel's final verdict, then, is that Napoleonic tyranny must disappear not because it is intrinsically evil, but simply because it has ceased to fulfill its "progressive" function.[6]

Hegel's Theory of Community

Hegel's answer to his natural rights predecessors of the seventeenth and eighteenth centuries is summed up under the concept of *Sittlichkeit* or "ethical life." By referring to the community under the category of social ethics, Hegel implies at least two things. First, "the standpoint of ethical life" is introduced in explicit contrast to "the standpoint of morality." Whereas Kantian *Moralität* applies to individuals qua individuals in abstraction from their specific social and political roles, ethics refers to the virtue of which a person is capable as a member of a specific community. Ethics refers not merely to the capacity for rational choice and action, but to "absolutely valid laws and institutions," to "custom," "habitual practice," and the "general mode of conduct" of individuals (Hegel, *PR*, 1972, par. 144, p. 105, par. 151, p. 108). Thus the sphere of social ethics has a content that is lacking from the individualistic and rationalistic moral theories of Hegel's immediate predecessors. Hegel's point is that despite the claim to speak universally and ahistorically, these moralities presuppose and are dependent upon a social context for their validity, and it is this context that the concept of *Sittlichkeit* intends to bring out.

Second, by speaking of a common way of life as a branch of ethics, Hegel harks back to the ancient world and its idea of the *polis.* Indeed, the German word for ethics, *Sitte,* derives from the Greek *ethos,* meaning the customary horizon or dwelling place within which life is lived. Ethics is, then, coterminous with politics, which is concerned with the just and the good or with the conditions of virtue or *eudaimonia.* The modern theorist who came closest to grasping this standpoint, Hegel says, is Montesquieu, whose "immortal" work introduced the concept of an *"esprit général d'une nation"* which approximates his own doctrine of ethical life (*NL,* 1975, pp. 128–29; *PR,* 1972, par. 261R, p. 161). Yet while Montesquieu, Hegel claims, operated with an essentially empirical and descriptive account of *esprit,* Hegel's is rational and necessary. It is not merely a covering concept used to describe the traditional rules, customs, and habits a society actually has. It is used to define what kinds of rules, customs, and habits it must have if a society is to be fully rational. Unlike Montesquieu, then, who never wholly freed himself from the methodological individualism of the Enlightenment, Hegel takes the bold step of predicating rationality not of individuals and their actions, but of the institutions and cultural context that makes action possible.

Hegel's reflections on the idea of political community underwent a number of changes. In his early works he tends to identify the community with an idealized image of the classical *polis.* For the Greeks, the *polis* was not merely a legal association created for the satisfaction of private ends but a way of relating that stressed shared values and common sacrifice even at the expense of individual interests. Hegel explicitly deprecates the emergence of the private realm, especially the longing for individual immortality, as a symptom of political decadence. "Only in moments of inactivity or lethargy could [the Greeks and Romans] feel the growing strength of a purely self-regarding wish," Hegel writes. "Cato turned to Plato's *Phaedo* only when his world, his republic, hitherto the highest order of things in his eyes, had been destroyed" (*ETW,* 1971, p. 155). It is clear that throughout these works Hegel, like many of his generation, looked forward to a revival of classicism as an antidote to the division and diremption of modern life. And yet, as I have tried to argue, Hegel's early flirtation with radical republicanism did not survive the failure of the French Revolution to achieve anything remotely resembling classical *polis* democracy.

Hegel's mature conception of a community is based upon an awareness of the complex differentiation of society into distinct classes or estates (*Stände*). His earliest awareness of the importance of social classes as means of linking persons together in morally satisfying ways is drawn from the essay on *Natural Law* as well as the unpublished *System of Ethical Life.* Here Hegel continues to work within a more or less classical paradigm taking for granted the Aristotelian division of society into the private world of the household (*oikos*) and the public realm of the *polis.*[7] While the former aims at the preservation and sustenance of life, the latter seeks to bring about the conditions of the good life or a life oriented toward the virtues. While the household serves the end of necessity, the *polis,* which grows out of necessity, serves the higher goals of nobility, honor, and excellence (Aristotle, *Politics,* 1280b; 1323a–25b).

Retaining Aristotle's distinction between *oikos* and *polis,* Hegel deduces the two major classes of the modern world. The first is the "absolute class" of "free men," or the military aristocracy (*NL,* 1975, pp. 99–100; *SEL,* 1979, pp. 152–53). This class is "absolute" because its activity is aimed not at the skillful production of artifacts, but at "the being and preservation of the entirety of the ethical organization." Following Aristotle he calls this class πολιτεύειν, or literally, "living in and with and for one's people" (*NL*, 1975, p. 100). The preeminent virtue of this class is courage exercised mainly in times of war. The second class Hegel identifies with the commercial bourgeoisie (*NL*, 1975, pp. 100–104; *SEL*, 1979, pp. 153–56). Reflecting his study of English political economy, Hegel for the first time speaks here of the "burgher in the sense of a bourgeois" (*NL*, 1975, p. 103; see also *PR*, 1972, par. 190R, p. 127).[8] This class does not risk its life, but typically enjoys the fruits of peace to pursue the accumulation of wealth and comfort. Its characteristic virtue is honesty and fair dealing.

Yet even as he attempts to graft the categories of ancient political philosophy onto the realities of contemporary economic life, Hegel profoundly alters their content. First, political activity, which Aristotle had said was something "natural" to man, Hegel seems to see as heroic or supererogatory. Rather than justice, the paradigmatic political virtue for Hegel is courage, which is portrayed by him as something that does not complete or perfect but must literally "overcome" or "transcend" nature. Accordingly, he tends to see true politics only in terms of war or moments of great national crisis. Thus he records his deep skepticism over any plans for a "perpetual peace," which could only result in a "corruption" of the "ethical health" of peoples (*NL*, 1975, p. 93; *PR*, 1972, par. 324R, p. 210; see also Smith, 1983, pp. 624–32).

Second, the commercial classes are no longer tied to the narrow circle of domestic economy, as in the ancient world, but emerge fully within the public sphere, within the domain of political economy. Unlike the ancient *oikos*, the contemporary commercial class has acquired a position of full equality with the political class. In the essay on the *Natural Law* Hegel, still under the influence of Gibbon's *Decline and Fall*, regards the emergence of the middle class as a product of the demise of the *polis* (*NL*, 1975, pp. 101–2). It would only be in the *Phenomenology of Mind*, at the end of Hegel's Jena period, that he would see the emergence of this class not as a symptom of political decline, but as an emancipation from the older ethical and religious restraints imposed upon individual conduct by the *polis* (*PHM*, 1966, pp. 229–40). Labor (*Arbeit*), which had previously been the work of slaves, has become an instrument of *Bildung* or the moral education and maturation of the species. The implication of this change is that future forms of community life will have to be sought not in the Platonic-Aristotelian model of the aristocratic citizen, but in the more mundane economic-poietic activity of the "burgher as a bourgeois."

It would not be for almost twenty years that Hegel, in the *Philosophy of Right*, would theorize the phenomenon of civil society (*bürgerliche Gesellschaft*) as a realm of activity distinct from the traditional categories of both *oikos* and *polis*. Civil society, Hegel makes clear, is a distinctively modern phenomenon (*PR*, 1972, par. 185R, pp. 123–24). It has no foundation in the ancient world where the "social," as we understand it, did not yet exist. In fact, civil society was still looked upon by Aristotle as identical with the political community (*koinonia politike*) (*Politics*, 1252a). Thus Cicero could define the citizen (*civis*) by the civic quality of "civility," or "knowledge of civic affairs and prudence" (*De Oratore*, 1.14.60). Indeed, this classical identification of civil with political even extends into the modern era where, for instance, Locke calls the seventh chapter of his *Second Treatise of Government* "Of Political *or* Civil Society" (Riedel, 1969, p. 143).

The novelty of Hegel's account of civil society rests on its distinction from the political state. While the state considers persons in their public roles as citizens, civil society considers them as "private persons" who aim only at their own interests (*PR,* 1972, par. 187, pp. 124-25). Civil society, then, takes its point of departure from human beings considered simply as creatures of "need," who see one another as means to the satisfaction of their private desires. Civil society is bound together neither by the close-knit ties of sentiment that bind the family, nor by the more disinterested forms of public altruism that bind the state. Not only, then, does civil society present itself as a "system of needs" with its increasingly complex division of labor, but it has given rise to a new "science," namely political economy (*PR,* 1972, par. 189R, pp. 126-27). It is to the lasting credit of this science that has "arisen out of the conditions of the modern world" to have discovered the laws governing "mass relationships and mass movements in their complexity and in their quantitative and qualitative character." Hegel even compares the insights of social theorists like Smith and Ricardo to the discoveries of Kepler and Galileo, who found an orderly pattern to the solar system which displays to the naked eye only "irregular movements" (*PR,* 1972, par. 189A, p. 268).

What defines civil society in the *Philosophy of Right* as a *civil* society is its division into various classes and estates, each with its own distinctive outlook, interests, and way of life. These estates—the peasantry, the business class, and the "universal" class of state functionaries—provide the crucial links or "mediations" between the "natural society" of the family and the more abstract universality of the modern state. Realizing that in the modern world the state has become too large and impersonal to serve as an object for the citizen's immediate affections and devotions, such opportunity for civic virtue and community mindedness as exist will have to come from these features of civil life. Like Montesquieu before him and Tocqueville later, Hegel understood the crucial role played in modern society by the "corporations," or intermediary groups. These intermediary bodies are especially important not only because they prevent the individual's submergence in the state or some abstract general will, but also because they provide some context out of which capacities, talents, and abilities may grow (*PR,* 1972, par. 255R, p. 154).

Hegel's theory of civil society, then, seeks to provide us with forms of civic virtue and shared values that might not, on first glance, appear to be forthcoming. It is a part of the "ethical" (*sittlich*) life of the community and plays, therefore, an important pedagogical or tutelary function in developing the individual's capacities for moral fulfillment and self-realization (*PR,* 1972, par. 187R, pp. 125-26). It is a kind of school for virtue which, as I understand it, seeks to chart a course between two equally powerful alternatives.

The first is the market understanding of politics adopted by theorists from Hobbes to Anthony Downs, which sees human beings as essentially egoistic calculators of pleasures and pains who regard political activity as a necessary evil to be endured for the sake of peace and, above all, "comfort" (*PR,* 1972, par. 191A, p. 269). This view of politics, which Hegel's Marxist critics attribute to him with an otherwise rare unanimity, is rejected emphatically by Hegel. While he regards the market and the liberation of individual self-interest from the constraints of communal life as an "achievement" of modernity, he is highly critical of those commercial republics, like England, where everything is judged in terms of exchange value alone. The particular virtue of civil society is that it provides a complex, articulated social structure within which human abilities can develop. The fact that we cannot develop in every direction does not indicate that Hegel's views were "one-dimensional" or that he had an insufficient grasp of our protean nature. Virtue, for him, is developed in the conscientious and single-minded devotion to a single craft or activity (*PR*, 1972, par. 207, p. 133). Civil society provides the context within which one can not only find fulfillment in one's work, but can help to forge satisfying corporate and communal ties with others.

The second extreme is the democratic or participatory style of politics represented by Rousseau and the French Revolution. Democratic politics, we have seen, is couched initially in the demand for radical freedom or autonomy. This is not merely the freedom of the bourgeois who wants to pursue his own interests in the market place; it is the freedom of the citizen who desires liberation from private interests altogether. Radical democracy assumes that no one is free until everyone participates in the collective decision making of the community, or in the words of one of Hegel's communitarian rivals, until "the state and the constitution" are "ruled by a genuine communal spirit . . . indissolubly united by the holy chain of friendship" (*PR*, 1972, p. 6). Hegel rejects radical politics for presupposing what no longer exists. A community is not the product of the general will; rather, it presupposes an already existing community of opinions and customs that in the modern world cannot be assumed to exist. Attempts to create such a community can only result in terror, as in the French Revolution, or in the forced homogenization of society that one finds in contemporary totalitarian regimes. In the final analysis Hegel agrees with Oscar Wilde's dictum that the problem with democracy is that it simply takes up too many evenings.

Hegel's purpose, then, as Shlomo Avineri (1972, p. 83) has said, was to find a middle ground between Hobbes and Robespierre, between the market place and citizen virtue. Hegel believed he had found such a middle ground in the existing class structure, with its corporations and professional associations comprising civil society. These institutions help to prevent further atomization and particularization, but also seek to connect the individual with some wider and morally satisfying forms of social life without merely submerging his identity or "personhood" within them. They provide, as it were, a "second family" between the *gemütlich* ties of the natural family and the more abstract, universal bonds of the state (*PR*, 1972, par. 252, pp. 152–53). To be sure, civic virtue no longer imposes the kinds of severe moral restraints demanded by either classical politics or biblical teaching. The grounds of civil society are altogether more mundane: respecting the law and following what is regarded as upright behavior (*PR*, 1972, par. 150, pp. 107–8). But Hegel takes the unheroic character of civil society to be its advantage. It provides for the rational, if somewhat bloodless, patriotism that the modern state expects.

Notes

Earlier versions of this paper were presented at the 1985 annual meeting of the American Political Science Association in New Orleans, and at the John M. Olin Center for Inquiry into the Theory and Practice of Democracy at the University of Chicago.

1. The most important of these are Rawls (1971), Nozick (1974), and Dworkin (1977).

2. The assumptions underlying this model are spelled out in Hollis and Nell (1975, esp. pp. 47–64) and Barry (1978).

3. The main texts consulted here and the abbreviations by which they are identified in the text are as follows:

BR	*Briefe von und an Hegel* (1952)
LHP	*Lectures on the History of Philosophy* (1955)
PH	*The Philosophy of History* (1956)
PW	*Political Writings* (1964)
PHM	*The Phenomenology of Mind* (1966)
JR	*Jenaer Realphilosophie* (1969)
ETW	*Early Theological Writings* (1971)
PR	*The Philosophy of Right* (1972)
NL	*On the Scientific Ways of Treating Natural Law* (1975)
SEL	*System of Ethical Life* (1979)

4. Hegel's views on the French Revolution have given rise to a vast and inconclusive literature. See Habermas (1974, pp. 121-41), Hyppolite (1969, pp. 35-69), Lukács (1975, pp. 10-16), Ritter (1972), and Shklar (1976, pp. 173-79).

5. On the debt to Montesquieu see Althusser (1959, p. 49), Lukács (1975, pp. 371, 492), Mosher (1984, pp. 179-88), Planty-Bonjour (1974, pp. 7-24), and Shklar (1976, pp. 142-46, 157-58).

6. For further evidence of Hegel's "Bonapartism" see *BR* (1952, vol. 1, p. 120): "This morning I saw the Emperor [Napoleon]—this world soul (*Weltseele*)—ride through the town on horseback." See also *BR* (1952, vol. 1, p. 185), where he refers to Napoleon as "the great constitutional lawyer (*der grosse Staatsrechtslehrer*) in Paris." For his later views on Napoleon's constitution making see *PR* (1972, par. 274A, pp. 286-87) and *PH* (1956, pp. 451-53).

7. For a suggestive treatment see Arendt (1958, pp. 28-38).

8. The influence of political economy is the central thesis of Chamley (1963), Lukács (1975), Plant (1977), Riedel (1969, pp. 75-99), and Ritter (1972).

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NATIONAL ENTANGLEMENTS IN INTERNATIONAL GOVERNMENTAL ORGANIZATIONS

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There has been a growing propensity among states to associate together in international governmental organizations, or IGOs, for a variety of purposes. Why do states join IGOs, and what are the consequences for states of membership in IGOs? In this analysis, an explanation is sought, drawing on the theory of functionalism espoused by David Mitrany and others, taking into account the number of years a state has had sovereignty, level of technology, extent of party competition, and overall power. For Third World states, membership in IGOs is associated with enhanced economic performance. An increasing number of IGOs in the system appears to lessen the states' mean proneness to war. Functionalist predictions are upheld. But functionalism needs to be supplemented both for comprehensive explanations and as a prescription for the future. Already there are so many IGOs that it is difficult for states to control them, which could make them progressively irrelevant or even jeopardize their existence.

The global political system now contains more than 1,000 international governmental organizations (IGOs) of one type or another, and states are deeply entangled in this expanding web. Denmark heads the list by belonging to 164 IGOs; 19 states are members of 100 or more, and the mean number of memberships held by member states of the Group of 77, the Third World Caucus, is over 61. The entanglement of states and international governmental organizations has rapidly increased. The United Nations and its related agencies, the European communities, the Organization of American States, and the North Atlantic Treaty Organization are well known, but states have joined together and created a great many more formal institutional structures than those prominent examples. This study describes the growing web of IGOs, analyzes the propensity of states to join them, and assesses the broad consequences of a state's total IGO memberships for its economic performance and conflict behavior. It then

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

raises questions about the implications for international relations of a continuation of the multiplying entanglement between states and IGOs, particularly in light of the apparent resistance to this trend evidenced by the U.S. and British withdrawal from the United Nations Educational, Scientific and Cultural Organization (UNESCO).

Functionalism as an Explanation

The theory of "functionalism," developed by David Mitrany (1933, 1966) and his followers, remains virtually the only corpus of scholarship about IGOs that offers general explanations of why states create and join such organizations, and what the consequences of this would be. Mitrany and others also saw their version of functionalism as a prescription to guide the development of the global political system. Because their version of functionalism is the only theoretical persuasion available to guide this analysis, it will be the point of departure.

Functionalism maintains that states create and join international governmental organizations because of two broad historical tendencies that date from the nineteenth century: the extension and deepening of political participation within states, and the continual advance of technology. Functionalism argues that mass participation in political life will inexorably increase, that general populations everywhere are primarily interested in increasing their own standard of living, and that mass participation will make economic welfare the dominant concern of governments. Functionalism also argues that technology offers immense possibilities for improving living standards, but that international cooperation is essential to take full advantage of the opportunities provided by technology; states are simply too small. In the perspective of functionalism, IGOs are the consequence of political pressures and technological opportunities.

Functionalism would expect and prefer that the membership and mandates of an IGO be determined by the problem at issue; the overwhelming majority of IGOs would and should have limited memberships and specific mandates. The point of establishing an IGO, according to functionalism, is to facilitate international cooperation with respect to a specific technical issue, not to establish a general political authority with broad scope and domain.

Functionalism postulates that entanglement in a web of IGOs will make states less bellicose. Given the pressures to join IGOs, and the economic benefits presumably gained from membership and participation in them, states would be loathe to jeopardize these benefits by escalating interstate disagreements to violent conflicts that would inevitably destroy IGOs. Moreover, the increased opportunities for communication that IGOs provide should make it easier for states to avoid or settle disagreements before they reach the stage of violent conflict. States ought to have an incentive to reach agreement because, presumably, they have a common interest in economic expansion.

To what extent do the international governmental organizations in the contemporary global political system and the interaction between them and their member states conform to these functionalist tenets? How adequate is functionalism as an overall explanation for the pervasiveness of IGOs? Does membership in IGOs affect the performance and behavior of states in the way functionalism suggests? This analysis examines the validity of functionalist tenets and assesses the extent to which functionalism must be supplemented for a full comprehension of the phenomenon of IGOs. It also raises questions about the adequacy of functionalism as a prescriptive guide to the future development of the global political system.

The Universe of IGOs

As a first stage in the analysis, it is necessary to describe the web of international governmental organizations as it currently exists. The basic source for information about the number and characteristics of IGOs is the *Yearbook of International Organizations,* which is published periodically by the Union of International Associations (UIA) in Brussels.[1] The data used in this analysis are derived from the nineteenth edition, which was published in 1981 (UIA, 1981).

Starting with the nineteenth edition, the *Yearbook* divides the organizations that are included into two major categories and four subcategories. The number of IGOs included in each subcategory in the 1981 *Yearbook* are:[2]

I. Conventional International Bodies	
A. Federations of International Organizations	1
B. Universal Membership Organizations	31
C. Inter-Continental Organizations	48
D. Regionally Delimited Organizations	264
II. Other International Bodies	
E. Emanations and Semi-Autonomous Bodies	405
F. Organizations of Special Form	287
G. Internationally Oriented National Bodies	39
H. Inactive or Dissolved Bodies	26
Total	1101

In addition, seven organizations listed in the *Yearbook* had been proposed, but were not yet in existence in 1981. Including these, 1108 IGOs are listed in the *Yearbook,* 1,075 of which were active in 1981.

The only IGO included in subcategory A is the United Nations. The United Nations' specialized agencies and other similar agencies comprise subcategory B. The International Exhibition Bureau and the International Olive Oil Council are examples of the type of organization included in subcategory C. The European communities are the most prominent of the organizations included in subcategory D.

The 344 organizations included in subcategories A through D are indisputably IGOs. The 731 organizations included in subcategories E through G have a more ambiguous status. Subcategory E includes such organizations as the United Nations Conference on Trade and Development, which, though it is a creation of the United Nations, has a larger budget, staff, and program than most of the organizations listed in subcategories C and D. It also includes some bodies, such as the U.N.'s Joint Inspection Unit, that because of their small size or apparent lack of autonomy are more questionable cases. Organizations such as the Integrated Global Services System and the Joint Nordic Organization for Lappish Culture and Reindeer Husbandry are included in subcategory F. The first is an offshoot of UNESCO and the second of the Nordic Council, but both are more than suborgans, and in their characteristics resemble many of the organizations included in subcategories C and D. Subcategory G includes many of the joint ventures set up by the member states of the Council for Mutual Economic Assistance (CMEA), as well as such bodies as the Nigeria-Niger Joint Commission for Cooperation.

Because the distinctions between active IGOs included in subcategories B through G seem blurred, because in the aggregate the IGOs listed in the *Yearbook* as "conventional international bodies" and those listed as "other international bodies" have similar broad characteristics, and because information that is not available for all organizations is in general, proportionately equally available for those in both major categories, this analysis will usually treat IGOs collectively, without reference to the two categories. If those 344 organizations included in subcategories A through D alone were regarded as true IGOs, as those who favor strict criteria might prefer, the generalizations developed here using the larger universe of organizations about why states join IGOs and the consequences of membership would apply, though the numbers would obviously be smaller. How many "real" IGOs there are in the global political system is obviously a matter of definition; "reasonable" definitions yield numbers that are larger than 344 but less than 1,075.

True to the prediction and preference of functionalism, the overwhelming majority of the IGOs that are included in the data set and could be classified according to their function and membership have specific mandates and limited memberships: 96.8% have functionally defined specific mandates, and 80.6% limit their membership according to one or another criteria. Of the total, 54.1% have mandates related to economic matters, and 56.4% limit their membership according to geographic criteria.

The Dynamic Evolution of the Web of IGOs

The preceding description provides an initial guide to the nature of the web of IGOs, but it is necessary to go beyond this. A sense of the dynamic processes involved in the creation of the web of IGOs can be gained from an analysis of the past, and trends from the past can be projected to foreshow likely developments if these trends continued unabated.

The analysis can be structured conveniently according to four periods. The first period starts in 1815, the year the Napoleonic Wars ended and the first IGO, the Central Commission for the Navigation of the Rhine, was created. It ends in 1914 with the outbreak of World War I. The second period starts in 1915 and ends in 1939, with the outbreak of World War II. The third period begins in 1940 and ends in 1959. This is the period of World War II and the construction of the postwar international order. The final period starts in 1960 and ends in 1981, the last year for which data were available for inclusion in this analysis.

The rationale for breaking the post-World War II period in 1960 is that 17 states, the largest number ever, gained independence in 1960. By 1960, it was clear that colonialism was doomed and the nation-state system would be extended to the entire globe. As of 1959 there were 90 independent states in the global political system, 69 of which had been in existence in 1945 when the postwar period began. Between 1960 and 1981, 70 more would be added. Starting in 1960, decolonization fundamentally altered the global political system, at least in terms of the number of independent states included within it. The emergence of the new states led to an explosion in the membership of those IGOs that had come into existence before 1960 and—as will be seen—in the number of IGOs.

With the use of this periodization, several trends become apparent. Before examining these trends, however, it is important to emphasize that most IGOs are relatively recent creations. Of the 880 IGOs for which the date of founding is available, 94.1% were established after 1939, and 70.3% were established from 1960 through 1981. This pace of multiplication is astounding, and showed no sign of slowing. Indeed, more than 40% additional organizations were created in the decade of the 1970s ($n=354$) than were created in the 1960s ($n=250$).

The first notable trend is that progressively relatively fewer IGOs met the Union of International Associations criteria for being "conventional international bodies"; that is, for inclusion in the first four subcategories in the *Yearbook*. Sixty-five percent ($n=13$) met these criteria in the period 1815–1914, 59.4% ($n=19$) in the period 1915–1939, 47.1%

($n=98$) in the period 1940–1959), and 31.8% ($n=199$) in the period 1960–1981. By far the largest share of the increase in other international bodies was accounted for by IGOs in subcategories E and F, which tend to be organizations that owe their existence to decisions of organizations already existing.

Stating the trend in another way, with the passage of time IGOs created more and more offshoots. The obvious advantage of this practice is that often all that is required to create a new IGO is a majority vote, not a new treaty that would require signature and ratification to take effect. Governments of states may also believe that it will be easier for them to keep track of IGOs that are offshoots of other IGOs than those that are totally disconnected from any existing structure. Whatever the reason, a large portion of the increase of IGOs since 1960 has been in the UIA category "other international bodies." The pace of creation of IGOs in the UIA category "conventional international bodies" reached a peak in the 1960s when 110 were established; only 86 were created in the period 1970–1981.

A second notable trend is the significant difference between the distribution of types of membership criteria for the international governmental organizations that were founded prior to 1940 and for those that were founded starting that year. The proportion of IGOs founded before 1940 that have no criteria in their constitutions limiting membership to particular political, geographic, economic, or cultural groups of states is much higher than it is for IGOs founded later.

This does not mean that in the years since World War II began states have not formed a large number of IGOs with potentially universal membership. Twenty-five such organizations were founded prior to 1940, and 143 starting that year. The absolute number for the post-World War II era is impressively high. What it does mean is that after World War II began, limited-membership IGOs multiplied much more rapidly than those with potentially universal memberships. Starting in 1940, more than 80% of the IGOs that have been founded and were still in existence in 1981 limited their membership according to some criterion. In total, 682 limited-membership organizations were established during this period, and the real number is undoubtedly higher, since the UIA does not have founding dates for all IGOs. Geography has always been the criterion most frequently used for limiting membership, and this continues to be the case.

This trend should be interpreted in light of the fact that the sovereign states of the nineteenth and early twentieth centuries were much more homogeneous than they are in the late twentieth century. A universal-membership organization formed in the earlier years had a much less diverse membership then than it does now or would have if it were formed in the present period.

That the relative proportion of IGOs that could have universal membership should fall off is logical. It would not be surprising if there were some upper limit on the number of universal membership IGOs that could be included in a global political system, even though this limit might be flexible over time.

Another factor is that as decolonization proceeded in the post-World War II period, and the number of sovereign states grew at an explosive rate, the opportunities for creating limited-membership IGOs also expanded rapidly. Organizations could be created both among new states and among new and old states. International bodies could make possible the continuation and extension of activities that were organized within the framework of a single sovereignty in the colonial era.

A consequence of the trend favoring limited-membership IGOs in the post-World War II period is that organizations established from 1940 on tend to have significantly smaller numbers of members than those established prior to this date. The mean number of member states of IGOs in the latter category is 42.9, while that of those in the former category is 20.4.

To explore further the trend of an increasing proportion of limited-membership IGOs, it is useful to categorize states in order to see the extent to which different types of states have formed exclusive IGOs. A threefold categorization based on broad political and economic alignments divides states among those that are members of the Organization for Economic Cooperation and Development (OECD),[3] those that are members of the Warsaw Treaty Organization (WTO),[4] and those that do not belong to either of these organizations, a group that for this reason is called "Other."

These three categories are mutually exclusive and roughy place states into the groupings that are used in conventional political analyses. The members of OECD are those that are customarily referred to as the "West," and the members of WTO are those that are usually referred to as the "Soviet bloc" or group. The residual category of "Other" includes those states that are referred to as the Third World.[5]

Of the 563 IGOs for which membership information is available, 103 were comprised exclusively of states that were members of OECD; 28 exclusively of states that were members of WTO; and 178 exclusively of other states. Most of the Western IGOs were the basic agencies and offshoots of: the OECD, the North Atlantic Treaty Organization, the European communities, and the Nordic Council. Beyond the Warsaw Treaty Organization itself, the 28 Soviet-group IGOs were primarily derivatives of the Council for Mutual Economic Assistance (CMEA).[6] CMEA includes three non-European states, Cuba, Mongolia, and Vietnam, that for most purposes of this analysis are included in the "Other" category. In 1981 there were 38 IGOs comprised exclusively of CMEA members; these included the 28 comprised exclusively of WTO members.

The IGOs that were comprised exclusively of states in the category "Other" were less likely to be derivatives of other organizations than those comprised exclusively of OECD and WTO states. Of the IGOs comprised only of states in the category "Other," 58.6% were in the UIA category of "conventional international bodies," while only 47.1% of the OECD-only IGOs and 42.9% of the WTO-only IGOs were in this category. The economically more advanced Western and Soviet-group states have been refining their existing relationships through establishing additional organizations, albeit often subsidiary ones, while Third World states have been establishing relatively more new relationships and consequently more new primary organizations.

Since 1960, more IGOs comprised exclusively of states in the category "Other" have been established than IGOs of any other type. In the 1960s and 1970s, they accounted for about 40% of the IGOs formed in each decade. During the 1970s, these states began to create substantial numbers of IGOs in the UIA category "other international bodies"; they created 39 of these and 35 "conventional international bodies." By the 1970s, Western states were creating more than three UIA secondary-category IGOs for every one primary-category IGO that they established.

IGOs comprised solely of states in the category "Other" offer the greatest potential for growth in the near future. In 1981, only slightly more than one such organization existed for each state in this category, while there were more than three WTO-only IGOs and more than four OECD-only IGOs for each state in those categories. States in the category

Table 1. Distribution by Function of IGOs Comprised Exclusively of Members of Particular Political-Economic Groups Established from 1960 through 1981 (in percentages)

	Group[a]		
Function	OECD	WTO	Other
General	4.5	0	3.8
Economic	47.0	96.0	73.7
Social	47.0	4.0	18.8
Security	1.5	0	3.8
	100.0	100.0	100.1
No. of cases	66	25	133

Chi Square = 29.28
Cramer's Phi = .26
Sig. = .00

[a] OECD = Organization for Economic Cooperation; WTO = Warsaw Treaty Organization.

"Other" could expand considerably the number of "conventional international bodies" among themselves, and they could increase the ratio between IGOs of this type and "other international bodies," moving toward the level established by the Western states. The evidence of the 1960s and the 1970s is that states in the "Other" category are moving in these directions.

There were also 104 IGOs in existence in 1981 that were comprised of states from both the OECD and "Other" groupings; they did not include any members from the WTO group. Thirty-one of these organizations were established in the 1960s, in the immediate aftermath of decolonization, and 22 in the 1970s. Many of these organizations could be regarded as providing elements of the framework of the world market economy.

The third notable trend is a tendency toward greater differentiation and variety in the mandates of the IGOs that have been established since 1939. While this trend is not as pronounced as the other two, it nevertheless is important. In the 1940s and 1950s, an unprecedentedly large number of organizations that were established—28, or 13.5% of the total—had mandates dealing with security. Starting in the 1960s, progressively larger numbers of newly formed IGOs had mandates in the social field. This expansion of the numbers of IGOs with mandates to deal with social issues was most marked among OECD-only IGOs, although the "Other" group also moved to establish relatively more IGOs with social mandates. The record of the Soviet group is somewhat different. Only one of the 28 WTO-only IGOs has a mandate to deal with social issues; it was established in the 1970s. The Warsaw Treaty Organization's mandate is security; the remaining 26 (92.9%) of the WTO-only IGOs have economic mandates. Table 1 shows the distribution by function of the IGOs that were established starting in 1960 and were comprised exclusively of members of one or another group.

The growing differentiation of IGOs reflects a widely observed tendency toward specialization in political institutions. Security organizations were so relatively prominent in the 1940s and

1950s because the post–World War II political order was being created. This order has been relatively stable since those years, as is reflected in the fact that only 3.1% ($n=19$) of the IGOs established in the period starting in 1960 have security mandates. The increasing focus on social issues mirrors the focus on such issues that developed within states, particularly the advanced industrial Western countries, in the 1960s.

With their overwhelming concentration on economic issues, WTO-only IGOs stand apart from the general trend. Beyond the basic security commitment of the Warsaw Teaty, the Soviet-group states appear to have been almost inexorably drawn into economic cooperation, but they either have little desire or little necessity for institutionalized intergovernmental cooperation in other areas. The CMEA-only organizations are also heavily concentrated in the economic area (92.1%, $n=35$).

In sum, the web of IGOs in existence in 1981 was dense and complex. Although functionalist tenets accurately describe the broad characteristics of the web and its dynamic evolution, one must go beyond functionalism for a more detailed description and for a fuller understanding of the growth of the web. The basic dynamic forces in the global political system—the urge to create a new order in the aftermath of a destructive war and decolonization—had a strong impact on the processes and course of institution building, as one would expect. IGOs are, after all, instruments of states, and states are likely to follow policies in this sphere that are similar and related to those that they follow in other spheres. The evolution of the web of IGOs also reflects an internal development within states: the greater attention paid by governments to social issues. An important exception to this trend, however, is the paucity of WTO-only IGOs directed toward social issues.

The Propensity of States to Join IGOs

The analysis so far has indicated that states belong to varying numbers of international governmental organizations. Now that the web of IGOs has been described, this varying propensity of states to join these organizations can be analyzed in detail. Functionalist tenets again provide the point of departure.

To give an indication of the varying propensity of states to be members of IGOs, Table 2 lists the 26 states that held the highest number of IGO memberships in 1981. For each state, it gives both the total number of full and associate memberships in all categories of IGOs, and the number of full memberships in "conventional international bodies." Denmark's leading the list is explained by the fact that its unique position as a member of both the European communities and the Nordic Council gives it an unusual opportunity to belong to a large number of "other international bodies."

Fourteen of the 26 states, including all of those in the top 10, are from Western Europe; 3 are from Eastern Europe and 3 from Asia and the Middle East; 2 are from North America and 2 from Latin America; and 1 is from Africa and 1 from Oceania. These membership data reflect the fact that the IGO web is most dense and complex in Western Europe, where it began with the creation of the Central Commission for the Navigation of the Rhine in 1815. What is most impressive about this list of 26 states and the participation of states in IGOs more generally, however, is the extent to which the web has become global. Even Vanuatu, which just gained independence in 1981, held 11 IGO memberships that year, 4 of which were in "conventional international bodies." Joining IGOs has become among the first actions that governments take as soon as sovereignty is gained.

Functionalism argues that states will be

Table 2. States with the Highest Number of IGO Memberships

Rank Order	State	Full and Associate Memberships in All Categories of IGOs	Full Memberships in UIA[a] Principal Category IGOs
1	Denmark	164	91
2	France	155	95
3	Norway	154	86
4	Sweden	153	87
5	United Kingdom	140	83
6	Finland	139	78
7	Federal Republic of Germany	135	83
8	The Netherlands	131	82
9	Belgium	127	77
10	Italy	124	72
11	United States	122	67
12	Spain	113	76
13	Canada	110	69
14	Japan	106	63
15	Iceland	105	54
16.5	Australia	104	67
16.5	Soviet Union	104	67
18	India	102	61
19	Brazil	100	60
20	Poland	99	69
21	Algeria	96	57
22.5	Austria	95	62
22.5	Yugoslavia	95	58
25	Egypt	94	60
25	Mexico	94	56
25	Swizerland	94	65

[a]UIA = Union of International Associations.

propelled to join IGOs because popular pressures to increase living standards will lead their governments to engage in international collaboration to take advantage of the opportunities that technology offers to respond constructively to these pressures. Following this argument, one would expect that states with more opportunities for popular pressures to be expressed and at higher levels of technological development would belong to a relatively greater number of IGOs. The extent of party competition is an appropriate indicator for the first variable, and per capita gross national product (GNP) is an appropriate indicator for the second. For this analysis, Freedom House's fourfold classification of states in 1980 as (1) multiparty, (2) dominant-party, (3) one-party, and (4) no-party (Gastil, 1981) is used to indicate the extent of party competition. The per capita GNP figures used are those for 1980 published by the World Bank in its *World Bank Atlas 1982* (IBRD, 1982a).

True to the functionalist argument, party competition and per capita GNP do predict IGO memberships.[7] An ordinary least squares regression with these two independent variables produces an equation with $R^2 = .29$ ($n=160$, sig.$=.00$). The functionalist argument is supported, but less than 30% of the variance is explained, which leads to a quest for further factors that might influence the propensity of states to belong to IGOs.

Since the phenomenon of decolonization proved so important in the growth of the web of IGOs, it could also be an important factor in explaining the propensity of states to belong to IGOs. At the most basic level, the longer a state has had sovereignty the more opportunities it would have had to join IGOs. Since more than 90% of the IGOs in the global political system were established after World War II, it seems appropriate to assume that the exact order of states coming to independence in the nineteenth century or early twentieth century, or even earlier, would have little bearing on their propensity to belong to IGOs in the late twentieth century. Thus years of sovereignty—or of membership in the global political system—are measured starting in 1945. Since the data used in the analysis are for 1981, this independent variable has values from 0 to 37, 0 being for territories that did not have sovereignty in 1981, and 37 being for states that gained sovereignty in 1945 or earlier.

Traditional thought about world politics has always accorded special status to great powers. Given their presumed propensity to be extensively involved in world politics, one would expect greater powers to have more IGO memberships than lesser powers. GNP is the most convenient single indicator of power. The GNP figures used here are also taken from the *World Bank Atlas, 1982.*

Adding date of entry into the global political system and power to the two independent variables used previously sharply improves the explanatory power of the ordinary least squares equation. With the four variables included, the equation is:

$$\begin{aligned} \text{IGO Memberships} = {} & \underset{(5.33)}{36.29} + \underset{(.14)}{1.43}\ \text{System Years} \\ & + \underset{(.00)}{.002}\ \text{Per Capita GNP} \\ & - \underset{(1.34)}{4.43}\ \text{Party Competition} + \underset{(.00)}{.00002}\ \text{GNP} \end{aligned}$$

$R^2 = .61$; standard error of estimate = 1961; and level of significance = .00. (The partial coefficients are unstandardized; numbers in parentheses are standard errors.)

This equation explains more than 60% of the variance.[8] The relationships are shown in Figure 1.

The four independent variables are listed in the equation and shown in Figure 1 in the order they were selected in a stepwise regression procedure. Years in the global political system is the most powerful predictor. An ordinary least squares regression equation with it as the sole independent variable yields $R^2 = .46$, larger than the R^2 of the equation utilizing the two independent variables suggested by functionalist tenets.

The effects of the four independent variables in the regression equation can be understood more clearly by interpreting the coefficients in terms of the measurement of the independent variables. According to the equation, each additional year a state has been in the global political system increases its total number of IGO memberships by 1.4; each additional 100 dollars in per capita GNP increases the total by 1.8; each step toward a multi-party system increases the total by 4.4; and, each additional 10 billion dollars in GNP increases the total by 1.6. Since the measure for party competition is ordinal, it is possible to obtain a better estimate of the effects of differ-

Figure 1. Factors Influencing the Propensity of States to Join IGOs (N=160)

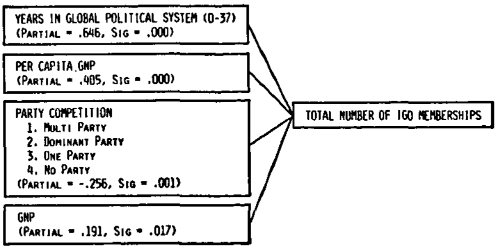
YEARS IN GLOBAL POLITICAL SYSTEM (0-37)
(PARTIAL = .646, SIG = .000)
PER CAPITA GNP
(PARTIAL = .405, SIG = .000)
PARTY COMPETITION
1. MULTI PARTY
2. DOMINANT PARTY
3. ONE PARTY
4. NO PARTY
(PARTIAL = -.256, SIG = .001)
GNP
(PARTIAL = .191, SIG = .017)
TOTAL NUMBER OF IGO MEMBERSHIPS

Table 3. Propensity of States Belonging to Particular Political-Economic Groups To Join IGOs (in percentages)

Relationship of Actual to Predicted IGO Memberships	Categories of States Members of OECD	Members of WTO	Other
Underachievers	16.7	0.0	17.0
Normal Achievers	50.0	100.0	72.1
Overachievers	33.3	0.0	10.9
	100.0	100.0	100.0
No. of cases	24	7	129

Chi Square = 11.988
Cramer's Phi = .194
Sig. = .017

ing degrees of competition by creating dummy variables. When this is done, it is clear that the largest difference is between those states that have multi-party systems and the others, but that there is also a fairly important difference between states that have a one-party system and those that do not have any parties.

The regression equation can be used to establish a "predicted" number of IGOs to which a state would be expected to belong on the basis of its characteristics according to the four independent variables. To give some examples, using the predicted values resulting from the regression equation, Denmark would be expected to be a member of 109 IGOs rather than the 164 to which it actually belongs; the United States a member of 145 rather than 122; Ethiopia a member of 71 rather than 46; and Algeria a member of 55 rather than 96.

It is instructive to examine the characteristics of the "outlying" states; those that belong to a total number of IGOs that is more than a standard deviation either above or below the predicted number. Those in the former category can be regarded as "overachievers," and those in the latter as "underachievers." Table 3 shows the relationship between the three categories of states that were used previously in this analysis and the predicted and actual membership in IGOs. Not surprisingly, the members of the OECD are more likely to be overachievers than are the members of the Soviet group or the "Other" category. The bulk of the OECD members are in Western Europe where the IGO web is most dense. Stating the relationships shown in Table 3 in a different manner, the mean actual number of IGO memberships of OECD members ($n=24$) was 116, while the predicted mean number was 106. The mean actual and predicted numbers of IGO memberships for members of the Warsaw Treaty Organization ($n=7$) were respectively 88 and 86, and for "Other" states ($n=129$), 59 and 61.

That there appear to be pronounced relationships between the type of economic system a state has and its propensity to belong to IGOs is more worthy of note. Table 4 shows the relationships. The categorization of states as having capitalist, state capitalist, mixed capitalist, mixed socialist, and socialist economies is taken from Freedom House's assessment as of 1980 (Gastil, 1981). Table 4 shows that states with mixed capitalist economies are particularly likely to be overachievers. This group includes not only Denmark, Finland, the Netherlands, Nor-

Table 4. Propensity of States with Various Economic Systems to Join IGOs (in percentages)

Relationship of Actual to Predicted IGO Memberships	Type of Economic System				
	Capitalist	State Capitalist	Mixed Capitalist	Mixed Socialist	Socialist
Underachiever	16.7	16.7	6.2	5.9	28.0
Normal achiever	75.0	69.0	43.8	88.2	64.0
Overachiever	8.3	14.3	50.0	5.9	8.0
	100.0	100.0	100.0	100.0	100.0
No. of cases	60	42	16	17	25

Chi Square = 19.690
Cramer's Phi = .2791
Sig. = .012

way, Sweden, and the United Kingdom, but also Senegal and Tunisia. This tendency fits with the technocratic, planning undertones in functionalism.

Beyond these eight states and France and Italy, which have state capitalist economies, the overachievers are, like Senegal and Tunisia, developing countries. They include five countries with capitalist economies (Cameroon, Ivory Coast, Kenya, Niger, and Upper Volta), four with state capitalist economies (Ghana, Mauritania, Nigeria, and Bangladesh), one with a mixed socialist economy (Sudan), and two with socialist economies (Algeria and Tanzania). These developing countries have in common relatively activist foreign policies.

Table 4 also reveals a slight tendency for states with socialist economies to be underachievers. An examination of the other characteristics of the seven states in this category—Afghanistan, Albania, Ethiopia, Laos, Mongolia, the People's Democratic Republic of Korea, and the People's Republic of China—suggest that political factors may be as important as the type of economic system.

Again, functionalism has provided the first step toward understanding the propensity of states to belong to international governmental organizations, but as in the task of describing the web IGOs, it needs to be supplemented with more traditional explanations of international politics.

IGO Memberships and the Performance and Behavior of States

Does belonging to international governmental organizations make a difference to the economic performance and conflict behavior of states, as functionalist tenets asserted that it would? Establishing causality definitively with respect to such complex issues is beyond the scope of this analysis, yet it is possible and worthwhile to examine the question and reach some tentative judgments about the extent to which expectations based on functionalist tenets have been met.

The essential tenets of functionalism with respect to the performance and behavior of states are simply the assertions that joining IGOs would enable states to improve their economic welfare, and that entanglement in a web of IGOs would tend to make states less bellicose. There is ample evidence relevant to these assertions. Their merits can be examined by exploring the association between the number of IGO memberships held by states and their economic performance and conflict behavior. The absence of an association would lead to a presumption against the validity of functionalist expectations. Although finding an association would not establish causality along the lines posited by functionalist tenets, it would create a favorable disposition toward the validity of these tenets.

Since, according to functionalism, states join international governmental organizations primarily in the expectation of obtaining economic benefits, it is appropriate to begin an analysis by examining whether or not better economic performance is associated with IGO membership. Economic performance can be and is measured in a variety of ways that are linked, but are not perfectly correlated. The ultimate goal driving the functionalist theoretical structure is increased individual economic welfare. The measures used here—growth in gross domestic product (GDP), manufacturing, and exports and imports, and improvements in terms of trade—are related to that goal, because they could make possible improvements in individual welfare. They are, at the same time, aggregate phenomena on which IGO participation could have a more direct and immediate impact than individual welfare.

The test used in the analysis will simply be to see if larger total numbers of IGO memberships are associated with higher rates of growth in GDP, in the manufacturing sector, and in exports and imports, as well as with better terms of trade. To posit that they should be is the baldest statement of functionalist tenets; putting the matter so starkly facilitates testing these tenets. The data used are from the World Bank's *World Development Report, 1982* (IBRD, 1982b). These and other available data could sustain more sophisticated analyses, but the crude tests used here can give adequate first approximation answers.

For the purposes of this analysis, the relative rank order of states according to the number of IGOs to which they belonged is assumed to have remained relatively constant during the years from 1960 through 1981. The only measure of IGO membership is that for 1981, it is assumed that the rank order pertaining in that year was not very different in the preceding 20 years. The statistical tables in the several editions of the UIA *Yearbook* support this assumption. Thus if a state belonged to a large number of IGOs in 1981, it most probably belonged to a relatively large number throughout the period.

With respect to the member countries of the OECD and WTO, the data show no association between number of IGO memberships and economic performance. States in both categories belonged to a relatively large number of IGOs. Compared to the universe of states and territories in the global political system, all OECD and WTO members were relatively affluent. For these states, factors other than the relative number of IGO memberships appear to have been the predominant determinants of economic performance in the two decades starting in 1960; the relative number of IGO memberships seems not to have made much difference.

For states in the category "Other," in contrast, the number of IGO memberships is associated with all measures of economic performance used in this analysis: growth in exports and imports, and improvement in terms of trade. The last factor is measured by the index number for the terms of trade in 1980, an index which used 1975 as the base. Table 5 shows the results of ordinary least squares regression equations measuring the association between the total number of IGO memberships that a state held and the various measures of economic growth. The associations are significant, and the range of variables covered is impressive. Both GDP growth in the 1970s and manufacturing growth in the 1970s are also significantly correlated with the years that a state has been in the global political system, and adding this

Table 5. Association Between Total Number of IGO Memberships and Various Measures of Economic Performance: States in the Category "Other" ($n=129$)

Measure of Economic Performance		OLS Regression Equation[a]	R^2	Standard Error of Estimate	Level of Significance
GDP Growth, 1960s	=	− 1.219 + .072 IGOs (.706) (.011)	.25	2.93	.000
GDP Growth, 1970s	=	− 1.788 + .078 IGOs (.663) (.010)	.31	2.76	.000
Manufacturing Growth, 1960s	=	− 1.849 + .071 IGOs (.835) (.013)	.19	3.47	.000
Manufacturing Growth, 1970s	=	− 2.911 + .094 IGOs (1.004) (.016)	.22	4.17	.004
Export Growth, 1960s	=	− 2.293 + .113 IGOs (2.005) (.031)	.09	8.32	.001
Export Growth, 1970s	=	− 1.819 + .053 IGOs (1.250) (.020)	.04	4.42	.007
Import Growth, 1960s	=	− 1.281 + .079 IGOs (.970) (.015)	.18	4.03	.000
Import Growth, 1970s	=	− 3.231 + .104 IGOs (1.553) (.024)	.12	6.45	.000
Terms of Trade, 1980	=	−31.402 + 1.581 IGOs (10.268) (.161)	.43	42.67	.003

[a] Coefficients are unstandardized; numbers in parentheses are standard errors.

variable to the ordinary least squares regression equations increases the R^2 in each case from .31 to .39 with respect to GDP growth in the 1970s, and from .19 to .22 with respect to manufacturing growth in the 1960s. In each case, however, more of the variance is explained by IGO memberships.

Given the predominance of IGOs with economic mandates and the fact that on the average 67.0% of the organizations to which states in the category "Other" ($n=129$) belonged were of this type—the average percentages for OECD ($n=24$) and WTO states ($n=7$) were respectively 65.7 and 77.7—extensive IGO membership implies extensive participation in an international political economy. The more IGOs a developing country belonged to, the more opportunities it would have to articulate its economic interests and to make efforts to defend these interests. It would also have more potential sources for economic and technical assistance.

Analyzing the coefficients in the equations in terms of the way in which the dependent variables were measured illustrates the indicated effects of IGO memberships. GDP growth is measured in average annual percentage rates during the two decades, the 1960s and 1970s. For states in the category "Other," during the 1960s the lowest average annual rate of growth was −2.0%, the highest was 24.4%, and the mean was 2.4%. During the 1970s the lowest rate was −9.2%, the highest was 12.1%, and the mean was 2.1%. The coefficients in the two ordinary least squares regression equations indicate that membership in an additional 10 IGOs is associated with an increment of more than .7% average annual increase in GDP during each of the decades.

Table 6. Mean Values for Various Measures of Economic Performance for States in the Category "Other" Belonging to Fewer or More than the Predicted Number of IGOs

Measure of Economic Performance	States Belonging to Fewer than Predicted Number of IGOs	States Belonging to More than Predicted Number of IGOs	% Difference Between Means
GDP Growth 1960s	2.5	3.7	51.7
GDP Growth 1970s	2.2	3.3	46.2
Manufacturing Growth 1960s	2.1	2.6	25.4
Manufacturing Growth 1970s	2.0	3.4	68.0
Export Growth 1960s	3.2	5.6	174.6
Export Growth 1970s	0.9	1.9	121.1
Import Growth 1960s	3.0	3.8	28.2
Import Growth 1970s	2.3	3.5	57.0
Terms of Trade 1980	49.2	76.5	55.5
No. of cases	65	57	

Viewed in this way, IGO membership had the greatest consequences for growth of exports during the 1960s and growth of imports during the 1970s.

States in the category "Other"—and indeed in the other two categories as well—as a group performed less well according to virtually all measures in the 1970s than they did in the 1960s. The differences in the associations between IGO memberships and most of the measures of economic performance suggest that in the difficult years of the 1970s, belonging to more IGOs was even more beneficial for developing countries than it was in the palmier years of the 1960s.

Another way of trying to assess the relationship of memberships in IGOs is to compare the economic performance for those states in the "Others" category belonging to more than the number of organizations predicted by the regression equation developed in the preceding section with those belonging to fewer than the predicted number. Table 6 gives the mean value for the same variables that were included in Table 5. In all cases the mean value is higher for those states that belonged to more than the predicted number of IGOs. This confirms the impression gained from Table 5 that the economic performance of states in the "Other" category was positively associated with the number of IGOs to which they belonged.

Demonstrating this association, of course, does not prove that belonging to more IGOs caused better economic performance. It could well be that particular economic strategies resulted in both better economic performance and relatively more IGO memberships. For example, pursuit of an outward-looking development strategy that emphasized production for and competition in the world market could be the underlying explanation for both phenomena. Unraveling the thread of ultimate causality, however, is exceedingly difficult: governments of states could be attracted to such strategies because of their extensive participation in international governmental organizations. Whether or not IGO memberships caused better economic performance, the strong association indicates that at a minimum they facilitated this.

This analysis of economic performance thus partially supports functionalist tenets, but not for the group of states with which functionalist prescriptions originally were most concerned: advanced industrial states. The findings indicate that if functionalist expectations about economic performance were valid, they may be more so for earlier rather than later stages of industrialization. They could also indicate that the marginal benefits of additional IGO memberships may decline, or that there may be a threshold beyond which further benefits do not accrue.

If these crude analyses regarding the economic performance of states give credence to at least a qualified version of functionalist tenets, what about the conflict behavior of states? The expectation based on functionalism would be that the more states became entangled in a web of international governmental organizations, the less inclined they would be to engage in interstate violence.

The measure of each state's inclination to engage in violence used here is the number of international—both interstate and colonial—wars in which the state has engaged since World War II, divided by the number of years that it has been independent during this period (Small and Singer, 1982, pp. 82–99). The result can be regarded as an index of proneness to war during the post-World War II period. The reasonableness of exploring the validity of functionalist tenets by examining the association between this index number and the total number of IGO memberships held by a state in 1981 again rests on the assumption that the rank order of states according to their IGO memberships has remained relatively constant over time: that those states that belonged to relatively more IGOs in 1981 also belonged to more earlier in the post-World War II period.

Testing the relationship in a bivariate manner reveals no association between the relative total number of IGO memberships held by a state and its proneness to war; belonging to a greater number of IGOs does not make a state less war prone. Nor is any association discernable if states are divided into the three categories of OECD members, WTO members, and "Other." Perhaps, however, this test is inappropriate.

It could be argued that functionalist expectations about conflict behavior should be applied to the system as a whole, not to the individual states that comprise it. Phrased in system rather than state-level terms, the argument would be that the more IGOs there were in the international system, the greater the opportunities would be for conflict resolution. The data support such an interpretation. From 1815 through 1914 the mean level of the proneness to war of the 46 states in the international system was .05; for the 65 states in the system during the interwar period it was .03; and for the 156 states in the system during the post-World War II period it was .02. These numbers reflect the fact that the period since World War II—when the number of international governmental organizations has rapidly multiplied and attained its present high level—has been relatively peaceful compared to other periods of modern era (Jacobson, 1984, pp. 190–192, 198–199). International governmental organizations, however, have clearly been only one of several factors, including the awesome destructiveness of modern weapons and bipolarity, that may have contributed to this relative peacefulness, and they may well not have been the most important one. The only broadly supported conclusion of this analysis may be the obvious one that now and in the near future IGOs by themselves cannot be counted on to eliminate violence from the global political system.

In sum, as functionalist tenets suggested, some states at least appear to gain economic benefits from participation in IGOs. The consequences of this participation for the conflict behavior of states, however, are more opaque; international war has not been eliminated, but IGOs may have contributed to diminishing its frequency.

Functionalism and the Future Evolution of the Web of IGOs

Functionalism provided a good point of departure for an examination of the multiplying entanglement between states and IGOs in the latter half of the twentieth century. It gave reliable initial guidance about the type of IGOs that would be established, the factors that would propel states toward IGO membership, and the consequences of IGO membership for the economic performance and conflict behavior of states. Yet functionalism fell short of providing a comprehensive explanation. It had to be supplemented by traditional explanations of international politics.

The evolving web of international governmental organizations has modified the global political system, as functionalism argued that it would, but it has not yet radically transformed this system, as functionalism hoped would happen. The radical transformation may yet come. In the meantime, however, international governmental organizations, in addition to modifying the political system, institutionalize aspects of traditional international politics.

The evolution of the web of IGOs has been affected by the broad historical currents of world politics, as well as by the dynamics foreseen by functionalism. Decolonization explains much of what has happened, and surely the exclusive organizations established by both Western states and members of the Soviet group owe their origins in part to the deep rift between these two groupings stemming from the Cold War.

Probably the multiplying entanglement between states and IGOs will continue in the immediate future, and perhaps even at the same dizzying pace. The complexity of modern life creates many pressures for states to establish additional IGOs. What has happened so far demonstrates the overwhelming sense of governments throughout the world that states no longer provide large enough frameworks for tackling pressing problems. As for modalities, there are not many barriers to the continuing generation of IGO offshoots. Finally, simply on the basis of mathematical possibilities, there are enormous opportunities for creating IGOs among Third World states. The regression equation developed here would predict that as the GNP and per capita GNP of Third World states rises, and if the competition of political parties within them increases, the number of IGOs to which each Third World state belongs will increase substantially. If the IGO web were to become as dense in the Third World as it already is in the West, the total number of IGOs would have to be multiplied several times.

The mere statement of these possibilities raises an issue that will have to be faced. The functionalist persuasion was enunciated and became popular before a dense web of IGOs existed; it provided an important impulse toward creating this web. The evolution of the web, however, cannot continue indefinitely on the basis of early basic ideas. States in the contemporary global political system on the average already belong to one IGO for every 356,490 of their inhabitants. The ratio is considerably more extreme for countries with numerically limited populations. For New Zealand it is one IGO for every 42,466; for Cape Verde, one for every 11,571; for Jordan, one for every 43,108; and for Costa Rica, one for every 28,013. Even for as populous a country as the United States, that belongs to fewer than the predicted number of organizations, the ratio is one for every 1,865,900. If the United States finds it administratively trying to formulate constructive policies for the organizations to which it belongs, as it does, what must the situation be like for countries that belong to proportionately more IGOs and have much smaller bureaucracies?

In most countries a relatively small number of bureaucrats along with a few delegates are charged with the responsibility of overseeing the work of and formulating policies for some 60 international governmental organizations. The impossibility of doing these tasks well in such circumstances is obvious. When the majority of states in an IGO are in such a position, the control of the direction of the organization can easily drift to the secretariat or to a minority of activist delegates who can muster majority support. States, and particularly those that provide the greatest financial support, can easily lose control. The United States and other Western countries allege that this has happened in UNESCO, and the U.S. and British withdrawal from UNESCO is a result of deep disagreement with the policies of the organization. Whatever the wisdom of the U.S. and U.K. decisions in this particular instance, there is a general problem. Somehow the multiplying entanglement of states and IGOs will have to take account of administrative realities and possibilities.

Functionalist theory expresses a preference for international governmental organizations becoming relatively autonomous from the states that comprise them. Functionalist theory sees such relatively autonomous IGOs gradually guiding states. UNESCO, which became relatively autonomous, could demonstrate the unreality of this vision. Given the fact of national control over resources, IGOs are at some risk when they ignore the preferences of the most powerful states. To do so may make them irrelevant to contemporary affairs, or, more seriously, could jeopardize their existence.

It is clear that creative ideas that go beyond functionalism are needed to guide the future evolution of the web of international governmental organizations. Such ideas will have to take into account and build on the empirical evidence presented here. Ways need to be discovered for effectively, constructively, and reliably engaging states in the web of IGOs that is continually being woven even more densely.

Notes

An earlier version of this article was presented as a paper at the 1984 convention of the American Political Science Association. The authors are grateful for the comments made during the discussion at the convention, especially by the discussants Francis Beer and Jeffrey Hart. We also appreciate the suggestions of Samuel H. Barnes, Philip E. Converse, Lawrence Finkelstein, Margaret Galey, M. Kent Jennings, Margaret Karns, Robert O. Keohane, John Kingdon, John Ruggie, and William Zimmerman. Gregory Markus, Laura Stoker, and Brian Wiersema provided valuable counsel concerning the statistical analyses. Harold K. Jacobson did some of his work on this article while he was a fellow at the Woodrow Wilson International Center for Scholars. He gratefully acknowledges this opportunity. The data on which the analyses are based are available through the Inter-University Consortium for Political and Social Research as ICPSR Study Number 8311.

1. The *Yearbook* contains information about both international governmental and international non-governmental organizations. The first edition of the *Yearbook* was published in 1909, and the most recent in 1983. The information contained in the *Yearbook* comes from responses to a questionnaire sent to the secretariats of international organizations by the Union of International Associations. The amount of information contained in the *Yearbook* about each IGO thus varies. There is a headquarters address for virtually all the IGOs that are listed, the date of founding for almost 80%, the member states for some 50%, the size of the staff for 12%, and the size of the budget for less than 3%.

2. Curiously, the numbers in the summary statistics included in the nineteenth edition do not correspond with the number of organizations that are actually listed. There are more IGOs listed in subcategories D through F of the *Yearbook*, and fewer in categories C and G, than the summary statistics indicate. In addition, some organizations that have been inactive or are dissolved are included in categories B through G of the listing, and as mentioned in the text, these categories also include some IGOs that had been proposed but were not yet in

existence in 1981. These latter organizations have been placed in a separate category, I, in the data set.

3. The members of the OECD are Austria, Australia, Belgium, Canada, Denmark, Finland, France, Federal Republic of Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, the United Kingdom, and the United States of America.

4. The members of the Warsaw Treaty Organization for the purposes of this analysis are Bulgaria, the Byelorussian Soviet Socialist Republic, Czechoslovakia, the German Democratic Republic, Hungary, Poland, Romania, the Ukrainian Soviet Socialist Republic, and the Union of Soviet Socialist Republics. Byelorussia and the Ukraine are not formally members of WTO, but they are members of several IGOs, and since they are part of the Soviet Union, which is a member of WTO, it seems appropriate to include them in this category.

5. It includes the 121 states that were members of the Group of 77 in 1981, and 50 other states and territories. Some of the 50 were not independent, but they nevertheless belonged to various IGOs, and thus should not be excluded from analyses. Were they independent, they probably would join the Group of 77, as indeed some of them have done after gaining independence in the years since 1981. The 50 also includes some small European sovereignties and other states such as Israel, Taiwan, and South Africa that are difficult to classify. All of these states need to be included in some group, and the latter group of states that are difficult to classify resembles the states in the "Other" category with respect to economic characteristics more than it resembles those in the OECD or WTO categories. The categories are used in the descriptions that follow, so that the inclusion of any particular state is not necessary: when IGOs composed exclusively of "Other" states are described, these bodies in fact seldom include those states that are difficult to classify.

6. The members of CMEA are Bulgaria, Cuba, Czechoslovakia, the German Democratic Republic, Hungary, Mongolia, Poland, Romania, the Union of Soviet Socialist Republics, and Vietnam.

7. In this analysis, the term "IGO memberships" is defined as including both full and associate memberships in organizations in both of UIA's categories. States' IGO membership using this inclusive definition correlate almost perfectly the total arrived at using various narrower definitions. The generalizations derived from analyses using the most inclusive definition would also be valid if a more restrictive definition were preferred.

8. Using states' full memberships only in IGOs included in the UIA category of conventional international organizations as the dependent variable yields a very similar equation. The ordinary least squares equation is:

$$\begin{aligned}
&\text{Full IGO membership in the UIA principal}\\
&\quad \text{category} = \underset{(3.33)}{18.66} + \underset{(.09)}{.67}\ \text{System Years}\\
&\quad + \underset{(.00)}{.001}\ \text{GNP per capita}\\
&\quad - \underset{(.84)}{2.58}\ \text{Party Competition} + \underset{(.00)}{.000009}\ \text{GNP}
\end{aligned}$$

$R^2 = .62$; standard error of estimate $= 12.26$; level of significance $= .00$.

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THE POLITICAL ECONOMY OF TRADE: INSTITUTIONS OF PROTECTION

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Domestic support for a liberal commercial policy in the U.S. rests on a commonly accepted set of rules and norms. These rules and norms, best characterized as a defense of free and fair trade, arise from two different traditions. The first originated as a political reaction to high trade barriers preceding the Great Depression. Central decision makers took as a lesson from the depression period that short-term political forces should not determine the level of state intervention into the market. The second, to protect against unfair trade, emanates from a long history of state support for industries that claim foreign producers are pursuing unfair predatory practices. Established in statute before the Depression, these rules did not fundamentally change in the postwar period. Together, these decision rules are used to interpret American trade protectionism.

In the postwar period American commercial policy has centered on the creation and perpetuation of a liberal trade regime. The tenets of that regime call for opening national borders to the free flow of goods, services, and capital; allocating primary responsibility for distribution to the market; and consigning the provision of collective goods to international organizations. Although all aspects of the liberal regime were never incorporated into international trade relations, the United States has promulgated this liberal design and attempted, where possible, to inculcate a neoclassical view of optimal trade relations into international practice.

Continued domestic support for this liberal economic regime is due to the acceptance of a set of rules and norms by the policy-making community. Although affected by nineteenth century British thought, America's version of liberalism is better described as a hybrid of ideas defending both free and fair trade. These ideas, encased in rules and norms, derive from both historical experience and institutional constraints. In particular, three behavioral rules determine the relationship between government and industry over the issue of protectionism.

Alternative Explanations

Currently popular are two approaches which offer alternative explanations for the extent of protectionism in the U.S.[1] The first, a structural approach, encapsulates protectionism within foreign policy goals. Structuralists argue that a nation's trade policy will reflect its foreign policy component; policy is associated with a nation's relative power position. Specifically, nations with preponderant power will have an interest in pursuing liberal trade (Keohane and Nye, 1977; Keohane, 1980; Krasner, 1976). The change in U.S. trade policy from support for high protective barriers against imports to its post-World War II support for openness is explained with reference to a change in America's relative international position. In the long term, policy always reflects the international configuration of power.

The second approach, an interest group or micro political economy analysis of politics, stresses the domestic component of trade policy. Analysis centers on the ability of interested groups to enact state policies that favor their product over foreign competitors in home markets. Although there is no clear consensus in the literature on the components of group activity necessary to assure translation of demands or interests into policy, there is agreement that explanation is rooted in group or economic interest, and in appropriate participation in the policy making process (Bauer, Pool, and Dexter, 1963; Lowi, 1964; Schattschneider, 1935). The shift in American policy in the 1930s to a less protectionist position derives from the same factor that explained protection in the first instance: a preponderance of social interest in one policy over the other.

With American economic and political power on the decline, and with American producers facing competition at home as well as in third markets, both approaches predict similar trends in U.S. trade policy. For one, increasing pluralist pressures from import-competing producers will lead to demand for policy change. For the other, the decline of American hegemony brings a decline in the benefits accrued from support for an open trading system. According to both approaches, the U.S. should no longer be interested in providing the collective goods necessary to keep international trade liberal. If economic openness is contingent upon either societal support or the existence of a hegemonic power, both protectionism and economic closure should have increased in the U.S. in the 1970s. It is argued correspondingly, that as the U.S. moves further away from support for free trade, the world again will embark, as in the 1930s, upon that "slippery slope" which leads toward both high protective barriers and intense competition over scarce markets (for example, see Greenhouse, 1984).

Herein lies a paradox. The empirical evidence over the entire postwar period cannot validate either position. Although there has been a growing interest in the revision of America's trade policy, there is insufficient evidence to support the contention that the U.S. is moving down the slippery slope toward protectionism. Rather, American central decision makers continue to be as committed to liberal trade today as they have been in any previous period in American history. The evidence for the continued existence of a liberal trade bias is overwhelming. The tariff schedule is at an historically low level. The last piece of liberal trade legislation, the Trade Act of 1979, which implemented the Tokyo Round Trade agreements, passed Congress with minimal objections. In the 1980s the executive office has not only dismantled programs to help industries adjust to import competition, but has withdrawn a number of orderly marketing agreements (OMAs) on the grounds they constrain trade. This is not to argue that no impediments exist to the movement of goods into the U.S. Nontariff barriers continue to protect certain American industries, but they are less prevalent and less restrictive than they were 30 years ago. Quantitative restrictions in the form of OMAs and voluntary export restraint agreements (VERs) have been increasingly used since the 1970s, but these agreements may not constitute evidence for a change in policy. Rather, given that the American executive is institutionally constrained as well as politically compelled to offer some response to beleaguered industries, OMAs may be understood to have arisen as an executive-centered alternative to more deterministic and protective forms of aid (Aggarwal, 1983; Yoffie, 1983).[2] In short, both theories lead an analyst to expect considerably more protectionism than is empirically evident.

If there has been so little change in America's general position on trade, why has an inordinate amount of intellectual energy and time been spent on worrying about the "slippery slope"? Since the early 1960s, dire warnings of an impending crisis in American trade policy have been heard from both academics and members of the government bureaucracy (Finger, Hall, and Douglas, 1982; Krauss, 1978; Viner, 1961). They warn of the potential for beggar-thy-neighbor policies; of the rise of powerful interest groups representing sunset industries no longer competitive internationally; and of unions which are responsible for high labor costs, the uncompetitiveness of American manufactures, and the slow adjustment of workers to changes in international demand. The continual reappearance of these predictions points to a series of flaws in the contemporary analyses of trade policy.

First, economists who investigate trade issues argue that aberrations in liberal trade result from pressures exerted by noncompetitive societal groups. Free trade conditions are the baseline. Deviations are due to political, albeit economically motivated, phenomena. Empirically, this leads to the study of the political strength of the less competitive traditional industries. Societal interest in changing general trade policy, as well as existing variations in tariff rates, is reduced due to the undue political influence of these sectors. Since most trade economists are not interested in a positive theory of government, little thought is given to the process by which interest group pressure elicits a government response. The outcome is assumed. Undue attention, then, focuses on the "interest" in aid, rather than on the amount and effectiveness of aid received. Individual cases of requests for aid are studied, but the larger picture of relative openness is ignored. This is a failing both for interest group and economic approaches to protectionism. They both equate trade problems with protection and look at state responses to requests for aid as putting out brush fires, rather than as any systematic or positive choices by the state.

Secondly, for those who look to the configuration of individual or group interests that comprise a political coalition as the backbone of state policy, the prediction of protectionism by the late 1960s derived from the analysis that liberalized trade was a Democratic party issue in the 1930s. Liberalism, along with a range of other issues closely associated with New Deal policies, was expected to be abandoned with shifts in the political climate. Whether or not the majority coalition has changed, neither Democratic nor Republican leadership in the 1970s and 1980s has organized a coherent anti-free trade platform. If anything is peculiar to this issue, it is that the Republicans have become more vigilant free traders than ever were their Democratic party equivalents.

Thirdly, those who argue that trade policy is predicated on international rather than domestic constraints predicted a rise in protectionism in the late 1960s. For them, the Vietnam War was both causal and indicative of new difficulties facing the U.S. Reflective of the change in America's international position, foreign policy in the 1970s was concerned with redefining vital interest and spreading the costs of maintaining the postwar alliance structure. As predicted, the U.S. did abandon the Bretton Woods system in 1971, and moved to a less U.S.-centered international monetary system. Such structural analysis would predict a deviation in the trade regime at least as substantive as that in monetary management. The absence of such a sharp break in policy has been explained away as due to the existence of a constraining trade regime (Krasner, 1979, 1982a, 1982b). Using regimes as variables, the hypothesis was introduced that the absence of trade shifts may reflect lags between interests and policy. Discontinuity between the two occurs when institutions, created to serve the powerful, take on a character of their own. However, there is yet to be a systematic theory that explains the longevity of institutionally based lags. Without such a theory, the concept of regime is a descriptive addition to structural analysis that does not abrogate the theory's general predictions.

In general, the overprediction of protectionism derives from the convergence of multiple theories that have given adequate explanations for the development of liberal policies, but have failed in their analysis of when policy shifts would occur. Although claims that protectionism has arrived, but in new forms, have been used to exonerate specific predictions, the more general claim that the U.S. is about to embark on that "slippery slope" is unfounded.

An Explanation for American Commercial Policy

How then should protectionism be understood in the U.S.? Protectionism, as a component of general trade policy, can be derived from a set of decision rules that are both historically and institutionally grounded. These decision rules reflect a set of beliefs held by central decision makers, and are indicative of a liberal bias. This liberal bias contains components of the intellectual tradition from which it arose, but mirrors at heart the American experience with trade policy making. The existence of a bias is manifest in policy in which feedback from both the domestic and international environment is muted.

The primary historic experience that set the stage for liberal trade policy in the U.S. was the Great Depression. Although the liberal policies adopted by the British in the 19th century had earlier found their way into the American debate on trade issues, it was the unquestionable failure of the Smoot-Hawley tariff of 1929-30 which allowed the fundamental restructuring of trade policy. High tariffs did not clearly cause the depression, but Smoot-Hawley came to be closely linked with its onset. The utter bankruptcy of the policy, advocating high trade barriers as the means for dealing with domestic economic malaise, set up two necessary conditions for fundamental policy change. First, institutional responsibility shifted from Congress to the executive. Congress had to approve this reduction in its traditional and constitutionally mandated role in tariff making. The prerequisite for this action was the consensus that trade mismanagement was linked to electoral misfortune. Secondly, the depression caused the demise of the then dominant analysis of trade policy, and created the environment for the acceptance of an alternative approach. Intellectual traditions take hold at moments when prevailing analysis is shown to be deficient. As the political community searches for a new and, it is to be hoped, more successful approach, alternative theoretical frameworks are seriously examined and can be used to explain and codify policy decisions. The change in ideas about the utility of one foreign economic policy over the other cannot be reduced to dominance of the

Democratic party. The change in ideas eclipsed any short-term change in government personnel. Rather, the 1930s was a period of fundamental restructuring of the role tariff barriers would play in solving American economic problems (Goldstein, 1983; see also Gourevitch, 1984; Odell, 1982; Taussig, 1914; and Tasca, 1938).

Economic decline breeds a myriad of explanations and remedies. The 1930s were no exception. Although the depression served to delegitimize protectionism, a coherent alternative position was not immediately available. Disenchantment with protectionism did not lead to the immediate opening of American borders. For policy makers, the lesson derived from the experience of the Depression was more modest. They concluded that the U.S. could no longer use tariffs to help non-competitive industries; the economic and political ramifications of ignoring market forces were too great. Politically this meant that short-term political forces, in the form of powerful and articulate industrial interests, could not dominate in trade matters.

This lesson, however, was not self-evident. The free trade position was argued by charismatic leadership in this period of Congress' search for an explanation for past failures. The passage of a piece of liberal trade legislation by Congress in 1934 did not herald the coming of America's liberal period. Rather, liberalism became ingrained as a policy bias because the return to abundance and growth was interpreted by intellectuals, especially academic economists, to be a result of the successive, although incremental, policy of reducing barriers to trade.

The American form of liberalism, however, incorporated pre-liberal norms, values, and, most importantly, institutions. Laws established to protect the American market from potentially predatory practices by foreign producers were in force by 1934. For example, antidumping and countervailing duty regulations had already been established to protect America's position both at home and in third markets. In institutional design, these statutes service an earlier U.S. concern with issues more associated with economic nationalism than with later ideas of national gains from liberalization. These institutions changed little, even as America changed its general policy thrust. The existence of these laws, which remained bureaucratic and not executive-centered throughout the postwar period, points to the major American deviation from liberalism's intellectual roots. In short, the U.S. was never willing to allow cheating; if American producers were forced to rely upon market forces in order to be competitive, so too must foreign producers (see Skowronek, 1982).

If punishment of those who are competitive due to infringements of the rules of the market is a central decision rule, why did the U.S. sanction both the European Economic Community's (EEC) and Japan's explicit deviations from the rules of the General Agreement on Tariff and Trade (GATT) trade regime in the 1950s and 1960s? The explanation most often given points to American security interests. High politics, which involved American security, took precedence over the low politics of economic policy making. Although it is correct to argue that American security interests and the liberal international economic design promulgated by the U.S. after the war are closely related, the onset of negative sanctions and political pressures on both the EEC and Japan for increasingly liberal policies after the mid-1960s did not signal a diminished security concern. Rather, the U.S. allowed Europe and Japan to cheat in the earlier period because their industries posed no threat to American producers. Cheating was allowed, but only in the abstract. As soon as producers from these regions became a threat, the U.S. did respond to defend home industries. Security issues served to mute this nationalistic response only when institutionally possible: most of these statutes force the use of technical, and not political criteria when a case is adjudicated.

The difference between the first decision rule of denying aid to noncompetitive industries and this second decision rule of not sanctioning predatory practices is that the former reduces to an issue of the relationship between state and society, while the latter is associated with earlier American foreign policy norms. At issue in the determination of whether to erect barriers to aid particular industrial sectors are a policy norm associated with the intellectual tradition of domestic laissez-faire and a policy rule adopted after the depression. In contrast, the decision rule which sanctions state intervention to forestall cheating derives from earlier mercantile norms. In short, liberal ideas existed side by side with an institutional structure created to service a far different set of foreign policy concerns.

A third and final decision rule derives from the policy-making style which has characterized American politics since the 1930s. With the Great Depression and the electoral dominance of the New Deal coalition, the norms of behavior between state and society over the issue of trade changed. Although intellectually liberalism was viewed as the correct direction for trade policy, its political counterpart of laissez-faire conflicted with the position that government must assure a hearing for plural interests. For reasons of domestic legitimacy, the American government had to look responsive. Although economic norms prescribed that constituent complaints go unheeded, political norms and existing institutions dictated the opposite.

The third decision rule for protectionism evolved from these dual demands. Policymakers in the U.S. cannot ignore domestic groups. However, that does not mean giving these groups what they want. The government needs to appear responsive, not be responsive. Thus, if an industry articulates a need for aid the response can be only symbolic support. Although the U.S. was wedded to a foreign economic policy that assured some level of industrial disquiet, the government's response to these complaints could be symbolic. The third decision rule stipulates that if aid is to be awarded it is to be as unrestrictive as is politically feasible.

The observation that few petitioning industries were awarded effective aid—aid that effectively countered foreign market domination—can be interpreted in different ways. The continuation of import pressure could be argued to be due either to mistakes, that is, the application of a technically incorrect solution, or to the inability to make these weak American industries competitive. Both explanations are confounded by the existence of more effective agreements such as those found in textiles. The lack of a greater number of effective programs for trade relief instead suggests a coherence in American policy. Industries do not receive effective aid inadvertently. Rather, central decision makers have acted whenever politically feasible to limit aid.

In the postwar period, U.S. central decision makers pursued liberal foreign economic goals even though their policies met with resistance. Potential opposition was finessed, not ignored. Although logic suggests that America's economic decline should be reflected in changes in commercial policy, the U.S. is in much the same position as Britain in the early part of this century: the national state is biased toward a liberal future (see Blank, 1977).

Making the Case for Free and Fair Trade

Since 1934 there has been a dramatic liberalization in America's trade barriers. This successive decline has been well

Table 1. Change in U.S. GNP, Imports, Exports, and Tariffs: 1948–1982 (%)

Years	GNP	Imports	Exports	Imports[a]	Tariff[b]
1948–1958	74	80	42	66	−20
1958–1968	93	160	93	176	+ 2
1968–1978	146	423	313	336	−47
1972–1982	162	339	325	274	−42

[a] Import figure if petroleum is excluded.

[b] Duties collected as percent of dutied imports.

Source: Calculated from *Historical Statistics of the United States: Colonial Times to the Present*, 1970, 1975, Part I, p. 224; Part II, pp. 884–900; *Statistical Abstract of the U.S.*, 1973, pp. 778–790; 1980, pp. 439, 872–884; 1982–83, pp. 833–844; 1984, pp. 448–449, 831–841.

documented (Destler, Sato, Clapp, and Fukui, 1973; Meier, 1973; Pastor, 1980; Preeg, 1973; Ratner, 1972; Tasca, 1938). This decline in trade barriers continued even as America's role in the world market shifted in the 1970s from its earlier dominant position. As displayed in Table 1, tariff reductions continue even as the growth of imports surpasses that for exports and the Gross National Product (GNP). In seven rounds of multilateral negotiations, American tariffs declined to an historically low 5% by the early 1980s. This was accomplished in the face of protectionist opposition fueled periodically by an overvalued dollar, by the perception of the use of unfair trade practices by trading partners, and by increasingly severe competition in the home market. Along with liberalization, American policy also has been characterized by increasing control of commercial policy by the executive, and by congressional delegation to a trade bureaucracy the authority to protect industries. In order to appease general and specific sectoral opposition, once the function of tariffs, Congress created and reinvigorated a myriad of mechanisms through which industries could articulate their needs.

Data are presented on five statutes that protect import-competing industries. Two deal with the problem of import penetration, three with the issue of unfair trade. All are argued to be facilitators of general trade policy; all contain elements that both serve and deflect domestic opposition. In the period studied, the trade agencies monitoring these statutes grew in size and scope as a result of both congressional intervention and increased societal demands for aid. Their authority was, however, constrained both by executive intervention and by inordinately difficult criteria for the granting of protectionism. Even when aid was given, it often was too little, too late. Of the industries that did receive aid, few were able to re-establish a competitive American market share.

Escape Clause Cases

Since instituted by executive order in 1947 and incorporated into statute in 1951, the U.S. has always included safeguard provisions into trade treaties and legislation. These provisions allow an industry seriously injured by imports to be exempted from an American trade agreement.

Escape clause action is usually initiated by industry, although either the president or Congress may start the investigatory process. To receive protection under this clause, industry must be proved to be adversely affected by imports, not by other market conditions or inefficiencies. The final decision to use the safeguard provision always resides with the executive branch, which with advice from the

International Trade Commission (ITC—old Tariff Commission), decides if aid is to be given and the type. The remedies available include a higher tariff, tariff quotas, import quotas, orderly marketing agreements, adjustment assistance, or some aid combination. Congress can overturn a presidential decision that differs from an ITC recommendation, but it has never exercised this power.

Escape clause provisions are not intended to provide long-term protection from imports. Rather, the intent of the relief is to provide "orderly adjustment to import competition" for the affected domestic industry. In particular, safeguard provisions are not intended to help industrials "that fail to help themselves through reasonable research and investment efforts, or that fail to take steps to improve productivity and other measures that efficient industries must continually undertake to remain competitive" (Annual Report of the President of the United States on the Trade Agreements Program, 1976, p. 42). Escape clause provisions therefore have always included limitations on the length of time an industry may receive relief.[3]

Adjustment Assistance

Adjustment assistance was first provided in the 1962 trade act as an additional remedy to be given using escape clause criteria. In 1974, the process by which firms and workers could receive such aid was eased, and the responsibility for administration was removed from the ITC and given to the Commerce and Labor departments. If a recommendation for adjustment assistance is received by the president, he must give aid. Adjustment assistance aid takes the form of weekly trade readjustment allowances, which may supplement a worker's unemployment insurance benefits or be used for retraining, job search or relocation benefits for displaced workers, and guaranteed loans and technical assistance for firms and communities affected by imports. This type of import relief, severely cut back under the Reagan administration, does not interfere with the U.S. free-trade posture. No further barriers to trade are constructed; rather, the government reimburses those adversely affected by the state's foreign policy goals.

Anti-Dumping Legislation

Enacted in 1921, anti-dumping is intended to prevent unfair foreign competition created by pricing items below their cost of production. A dumping ruling entails a finding of dumping by the Treasury Department (after 1979, by the Commerce Department), and a confirmation of serious injury to an American producer by the ITC. If both agencies report an affirmative finding, a special anti-dumping duty is assessed equal to the amount by which the price is below its foreign market value. There is no presidential discretion, but dumping charges originate and are awarded by the Treasury Department (by the Commerce Department after 1979).

Countervailing Duty Legislation

In existence since the end of the nineteenth century, countervailing duties are assessed whenever a bounty or grant, . . . i.e., a subsidy, is paid in a foreign country "upon the manufacture or production for export of any article or merchandise manufactured or produced in such country. . ." (*U.S. International Trade Commission Annual Report*, 1979, p. 9). A duty is levied equal to the net amount of the subsidy upon importation. After 1974, this provision covered both dutiable and non-dutiable merchandise. (Only non-dutiable determinations went to the ITC for an injury determination.) Under the 1974 trade act, the imposition of duties could be waived under a number of conditions by the secretary of the treasury. These conditions related to the effect of such duties on ongoing trade negotiations and on the foreign government's willingness to eliminate its subsidy program. Under the 1979 act, responsibility for countervailing duty investigations was transferred to the Commerce Department. All affirmative findings go to the ITC for a judgment on whether a domestic industry is being materially injured by reason of subsidized imports.

Unfair Import Practices

If foreign producers pursue unfair methods of competition which cause injury to an efficient domestic industry, their product may be denied entry. Under this statute, which has pertained mainly to patent claims, the ITC determines, subject to judicial and presidential review, whether the importer is violating the law.

The Findings

Three alternative approaches to the analysis of trade policy have been suggested. The first, an international structural explanation, predicts that policy outcomes would respond to changes in U.S. interests in the world. Variations should occur over time, mirroring changes in the international configuration of power. In general, American policy should be most liberal in the period from 1958 to 1968, and become increasingly less so in subsequent periods. The second approach, looking to domestic forces as explanation, expects the level of protectionism to increase with sectoral and/or group demands for aid. As issues of protectionism become more salient, Congress is expected to respond by becoming increasingly involved to insure that "hurt" industries obtain aid. The third approach offered here, looking to the role of ideas and institutions in policy making, expects American policy to be guided by a pro-free trade bias. The expectation is either that there is no relationship between time and aid, or, at minimum, that there is an inverse temporal relationship to that suggested by structuralists.[4] If aid is given, the expectation is that it will be symbolic, be oriented toward "buying off" potential opposition to further liberalization, or be the most liberal of a range of options facing a politically constrained executive. There should, however, be variation across types of aid. Aid to keep the market "fair" should have a higher success rate than aid which protects an uncompetitive sector.

In examining the five statutes detailed above, an increase in petition activity appears over time. As tariffs decreased and imports increased their market share in the 1960s and 1970s, American producers did react by petitioning the bureaucracy for protection. Also interacting with market pressures in the period examined are changes in the legislated procedures and criteria under which an industry could obtain aid. Along with market pressures, legal changes affect the utility of applying for aid. Although petition activity is self-evident when examining aid receipt, it too is skewed by the easing and tightening of federal laws.

Of the two types of import aid examined, adjustment assistance (AA) petitions increased most dramatically (see Table 2). From an average of 25 per year in the period from 1963 to 1974, they accelerated to a rate of 967 per year from 1975 to 1978, and to over 2,000 per year from 1979 to 1981. Comparatively, escape clause (EC) petitions were highest in the first and third periods examined (see Table 3). Between 1958 and 1962, and 1975 and 1978, they average 11 and 10 per year respectively. There was a sharp decline in interest in escape clause aid under the 1962 act, and again a decline appears to have occurred after passage of the 1979 Trade Act.

Of the cases involving market disruptions, petitions against unfair trade practices (UFT) increased from an average of 1 a year (1958–1962) to 17 a year (1979–

Table 2. Adjustment Assistance Cases: 1963–1981

	Number of Petitions		Yearly Average		Acceptance Rate	
Year	Commerce Department[a]	Labor Department	Commerce Department	Labor Department	Commerce Department	Labor Department
1963–1974	62	233	5	19	.37	.30
1975–1978	338	3529	85	882	.91	.45
1979–1981	1245	6213	623	2071	.81	.28

[a] Prescreened at regional offices, Commerce Department data only for 1979 and 1980.

Source: See Appendix.

1982) (see Table 4). Countervailing duty (CD) cases increased from a low of 1 a year to an average of 25 a year in the latest period. CD petitions began a sharp ascent in 1974, reaching their highest level between 1975 and 1978 (see Table 5). Comparatively, anti-dumping cases (AD) remained almost constant in the years between 1958 and 1982 (see Table 6). As with CD cases, the exceptional period followed the passage of the 1974 act. The average number of petitions between 1974 and 1979 almost doubled.

Comparison of the five statutes reveals that all had a dramatic increase in petition activity after 1974. In three (EC, AD, CD), that increase slowed down somewhat in the early 1980s. Petition rate, however, does not closely mirror the changes in market penetration presented in Table 1. Although interest in aid has increased over time, that increase has not looked the same across aid types. The reasons become more evident when each aid program is examined.

Escape Clause Aid

Three trends are noteworthy. First, the ITC rate approval has increased for escape clause cases over time. Compared with a 27% acceptance rate in the early 1960s, by the late 1970s the ITC was finding over 60% of the petitions they adjudicated worthy of import relief. This reflects both eased standards and increasing pressures on the American market. Technically, increasing numbers of indus-

Table 3. Escape Clause Cases: 1958–1981

Year[a]	Number of Petitions	Average Per Year	Number ITC Approved[b]	Number President Approved[c]	Acceptance Rate ITC	Acceptance Rate of President[d]	Acceptance Rate With ITC Remedy
1958–1962	56	11	15	8	.27	.14	.07
1963–1974	31	3	10	4	.33	.13	.03
1975–1978	40	10	24	9	.60	.23 (.20)	.03
1979–1981	13	4	8	5	.62	.38 (.23)	.08

[a] Organized by legislative periods.

[b] Includes Split Votes.

[c] An award of adjustment assistance alone is not considered aid.

[d] Number in parentheses indicates acceptance rate for industries not already covered by escape clause actions.

Source: See Appendix.

Table 4. Unfair Trade Cases (Section 337 of 1930 Tariff Act): 1958–1982

Year	Number of Petitions	Petitions Per Year	Number Settled	Number Accepted	Rate of Settlement	Rate of Acceptance
1958–1962	6	1	0	0	0	0
1963–1974	53	4	5	7	.09	.13
1975–1978	47	12	13	13	.28	.28
1979–1982	69	17	31	15	.45	.22

Source: See Appendix.

Table 5. Countervailing Duty Cases: 1958–1982

Year	Number of Petitions	Average Per Year	Number of Positive	Acceptance Rate
1958–1962	4	1	4	1.0
1963–1974	16	1	15	.93
1975–1978	149	37	45[a]	.30[b]
1979–1982	101	25	30	.30

[a] Thirty-three cases were waived.

[b] Eight percent if waived cases counted as negative.

Source: See Appendix.

tries were meeting the escape clause standard after 1974.

Secondly, although technically qualified to receive aid, a majority of these cases never received effective import relief. This finding is sustained by three types of data. First, the overall presidential acceptance rate reduced the effect of ITC positive findings. If only instances of new industries gaining escape clause aid are considered, the president reduced the rate of positive findings to 14% before 1974 and 26% thereafter. The low presidential acceptance rate may be the reason that petition activity declined after 1979 even though bureaucratic acceptances were increasing. Not only did the president veto the vast majority of cases his bureaucracy deemed in need of aid, but in cases where he accepted a need, he often did not accept the recommended remedy. Although the overall presidential acceptance rate is on the rise, so too is the use of non-ITC remedies. This tactic, which denies Congress an opportunity to overturn a presidential veto, was increasingly used after the mid-1970s. In over 50% of presidential decisions made after 1958, the

Table 6. Anti-dumping Cases: 1958–1982

Year	Number of Petitions	Average Per Year	Number Approved	Acceptance Rate
1958–1962	142	28	5	.04
1963–1974	286	24	62	.22
1975–1978	168	42	26	.15
1979–1982	113	28	28	.25

Source: See Appendix.

Table 7. Effects of Aid on Market Share for Escape Clause Petitioners: 1958–78 (%)

	Outcome[b]	
Change in Market Share[a]	Unsuccessful	Successful
Market growth or market constant	14.9 (28)	1.6 (3)
Market decline	64.9 (112)	18.6 (35)

[a]Computed by product categories. Market share is a measure of imports relative to the value of domestic shipments.

[b]The numbers in parentheses indicate the number of cases.

Source: See Appendix.

president accepted the positive ITC vote, but substantially deviated from the ITC's chosen remedy.[5] Thus, in over half the cases in which a positive escape clause decision was rendered, the president was able to undercut the clout of the ruling by tinkering with the amount of aid awarded. It is in this context that OMAs need to be understood. They are executive-centered remedies which weaken the more protectionist recommendations of the ITC. If these deviations in remedy are incorporated into the figures presented in Table 3, the overall acceptance rate for industries in the postwar period declines to under 10%, with the vast majority of these deviations occurring after the mid-1960s.

Thirdly, following the point above, when escape clause petitioners do get aid, import pressures seldom recede. Aid is often too little, too late, or too short-lived. When recipients are examined by product groups, they look no better off than those groups who were unsuccessful in garnering aid[6] (see Table 7). Under 2% of those who received aid can be portrayed as having maintained market share over the postwar period. In short, it appears that aid has had a minimal affect on the industries examined (see Baldwin, 1982). In 42% of the cases in which the president did not agree with the technical remedy suggested by the bureaucracy, industrial market share declined (see Table 8). This is not to argue, however, that the ITC remedy is guaranteed to succeed. In less than half of the instances of

Table 8. Market Share and Presidential Decision: Cases Decided between 1958 and 1978 (%)[a]

Market Share	President and ITC Agree	President and ITC Disagree
Decrease	26 (5)	42 (8)
Same or better	11 (2)	21 (4)

Source: See Appendix.

[a]The numbers in parentheses indicate the number of cases.

presidential-bureaucratic consensus did market share increase. It does, however, point to the institutional autonomy of the office of the president, and the limited history of government measures able to thwart import competition.

This review of the escape clause statute reveals three trends. First, although Congress has given much time and attention to appropriate escape clause procedures, changes incorporated into trade legislation have been undermined by presidential actions. Second, the role played by the ITC has been greatly diminished by presidential interference. The staff of trade professionals and their politically appointed commission become a much less interesting source of explanation for escape clause policy, since the decisions of the ITC are regularly overturned. Thirdly, in the aggregate, American policy with respect to the escape clause mechanism has not led to overt protection of industries unable to adjust to changed trade patterns. Using market share in 1963 as a bench mark we find that petitioning industries were not able to procure sufficient aid from the state to retain their market positions. Although in the short term some industries that were granted aid may have improved their market share, these industries continue to be overwhelmed by foreign imports.

These findings speak primarily against an interest-group analysis of protectionism. Although it appeared that the number of positive rulings increased with societal pressure, presidential intervention in the form and amount of aid granted and the inability of these industries to maintain their viability even with federal aid point to a problem with a pluralist analysis. These findings suggest that the government plays a far more autonomous role in granting aid than a society-centered theory would suggest. Petitioning parties may appear to gain a positive response, but this response may be merely symbolic.

Adjustment Assistance

Both in concept and form, adjustment assistance deviates from other aid types examined. Why then include this aid? Adjustment assistance is a central ingredient of the American trade program. Although adjustment assistance decisions are not tied to negotiations or concessions, industrial adjustment to international competition is central to the continuation of a consensus around a liberal trade regime. The American state cannot ignore groups who are adversely affected by national policy. The state can, however, either de-legitimize their claims or symbolically incorporate their needs into state policy. The industries most directly affected by liberalization are those that are unable to retool and produce competitive goods. These industries form a potential source of opposition to U.S. policy. Adjustment assistance is one policy that can mollify these industries. The costs of this institution are born by the state's budget, and are not passed on to the international community through changes in custom barriers. Because of this cost, the chronicle of petition activity decisions and payments can be seen as a measure of state support for liberal trade.

As Table 2 details, significant numbers of petitioners received aid. These numbers dwarf any of the other aid types examined. Although underused before 1974, transferring responsibility for AA cases to the Commerce and Labor Departments greatly enhanced the program's visibility. As opposed to other aid types, a majority of those who applied for aid did receive it. Although AA aid was consistently used by both Democratic and Republican administrations through 1980, the 1981 data reflects the decreased role such transfer payments have played in the Reagan administration. Worker aid receipts declined to 9% in 1981; in the previous two years aid receipt had run at 41% for worker cases.

Two complementary explanations may be suggested for this decline. First, in the 1960s and 1970s adjustment assistance served as a pressure valve. Potential opposition to liberalization was deflected through a system of transfer payments. Adjustment assistance helped keep the U.S. on its liberal path by reducing the need to give industries a protectionist trade barrier. Reagan's policy stance rejected both of these functions. The Reagan administration was committed to a reduced role for the state sector; governments should not use a system of redistribution as compensation for economic policy. Often characterized as an ideologue, Reagan argued for the benefits of trade unfettered by state involvement. Liberal trade did not need the assistance of an adjustment program. Secondly, the groups who had received the bulk of these transfer payments were groups whose political influence gave them a favored position in the Department of Labor. In an administration tied neither to organized labor nor to an aggressive Department of Labor, it is not surprising that such aid would cease.

Unfair Trade

A philosophical difference exists between cases of fair competitive trade that adversely affect an American producer, and cases of "unfair" trade that cause the same result. Belief in the long-term beneficial aspects of the market has led American decision makers to condemn foreign governments and foreign industries who interfere with "natural" market mechanisms, particularly when the goal of that manipulation is predatory. Since it is the relationship between the American state and private industry that is the model used to evaluate actions taken by other nations, no country is allowed to assist overtly home industries, or to interfere with consumer market preferences so as to undersell an American producer.

There are two problematic elements in the administration of laws which attempt to keep the market "fair." First, there is a problem in the discovery and definition of a violation. Aside from an apparent problem with data collection on the production of foreign goods, there is the more complex issue of what criteria are to be used to assess cross-national policies. States vary greatly in their philosophical and historical relationship to producers. The relationship found in the U.S. is not characteristic of that found in other polities. To use the U.S. case as a bench mark establishes criteria on the extreme end of a continuum. Other nations whose postwar recoveries depended on greater amounts of state participation find the American model a useless and impractical archetype. Further, it has been argued that the enforcement of American values on state-industry relationships not only interferes with a set of foreign policy goals that seeks prosperity in non-communist states, but translates into interference in other states' domestic policies. Such interference alienates allies, and in principle deviates from regime norms of sovereignty (Goldstein and Krasner, 1984).

Secondly, and related, a problem exists in the determination of intentionality. Foreign producers may without predatory intent engage in what Americans view as unfair practices. There is a dilemma in how to judge, for example, national pricing policies or subsidized research and development programs. In unfair trade cases, foreign practices were often not created to gain a market advantage, but merely reflected national goals. The difficulty of assessment, however, has not precluded the passage of legislation to protect American industry from such practices. On the contrary, unfair trade laws have a longer history than the other legislation studied. These two problems do serve, however, to undercut the potentially protectionist aspect of unfair trade legislation.

In examining the legislative histories of unfair trade statutes, certain trends emerge. First, in all three, some administrative flexibility has always existed. In administering AD and CD cases the Treasury Department was never constrained by legislation to enact a particular type of policy. In AD cases, Treasury Department officials, who by the mid-1960s held a pro-liberal bias, had the ability to eliminate cases by ruling no "just cause" for an investigation. Even upon a positive preliminary determination, the ambiguous nature of defining whether sales were "less than fair value" (LTFV) gave much latitude to investigators. Though defined by 1300 lines of text in the *Federal Register*, the nature of the assessment process still contains a large element of arbitrary evaluation (Davis, 1966). And even if LTFV sales were ruled to have occurred, the ITC until 1979 had a wide amount of discretion in its injury determination. In particular, the causes the ITC studied in making its final determination did not necessarily have to apply to findings used in making the injury determination. For instance, the commission could find no injury, based on a re-determination that the foreign producer was not, according to their judgment, dumping, and overturn the Treasury Department's ruling.

This discretionary nature of the injury determination and its potential for becoming an overly protectionist instrument was of central concern to foreign producers in the Tokyo Round of GATT negotiations. The anti-dumping code agreed to in Geneva incorporates the less protectionist notion of "material injury," not just any injury, by reason of less-than-fair value imports.

For CD cases, no delineation existed before 1979 on how an investigation should be conducted, or on the appropriate criteria for duty assessment. Rather, upon completion of an investigation the Secretary of the Treasury decided whether to impose the duty, and the appropriate "equalizing" amount. The 1974 and 1979 Trade Acts added an injury requirement in order to make U.S. law consistent with the GATT codes. In 1974, items admitted duty free became subject to countervailing duties only if the ITC determined that the practice had "the effect of substantially reducing sales of the competitive U.S. product in the United States" (*U.S. International Trade Commission Annual Report*, 1975, p. 3). The 1979 law extended this consideration to all items. The 1979 law also specifies for the first time procedures and guidelines for the new oversight agency, the Department of Commerce.

Unlike countervailing duties or cases of dumping, the administration of unfair trade did not change under the 1979 law. Although some free trade advocates have criticized the 1979 act for this (Marks, 1980), the organization of Section 337 had already included a number of "safeguards" against its application. In particular, both the judicial review procedure and the existence of a presidential veto have caused the law to be less frequently invoked.

Secondly, not only do all unfair trade laws exhibit administrative flexibility, but there is no clear criterion in the legislative histories of these laws for assessing whether they are getting more or less protectionist. In the countervailing duty and anti-dumping cases, the use of the injury determination in all cases after 1979 makes it more difficult to obtain aid, even with the change of departmental responsibility to Commerce. Injury tests in the past have undermined the use of these kinds of aid.

And finally, although the president holds no legal authority in CD and AD cases, office holders have increased their power over outcomes. Two cases exemplify this point. First, the imposition of the countervailing duty waiver under the 1974 act transferred final authority from the bureaucracy to the secretary of the treasury. As a presidential appointee and member of the president's cabinet, actions of the secretary may be assumed to be reflective of the president's own position. Secondly, the existence of an extra administrative mechanism such as trigger prices gives to the executive authority in dumping cases denied him by the legislature. In effect, the trigger-price system created an alternative to the Treasury route for anti-dumping petitioners, allowing the president to undercut a more deterministic Treasury finding (see Crandall, 1981).

When data are examined, none of the three types of aid shows a straight increase in acceptance rates over time. Although petition activity increased rather dramatically in the 1970s for all three types of aid, government response has not mirrored the increasing activity. In the case of countervailing duties, the acceptance rate has declined; for anti-dumping petitions, the rate rose after 1962, but has not significantly changed after that. The acceptance rate of Section 337s went up under the 1974 law only to decline after 1978.

In countervailing duty cases, the high rate of acceptance in the early period reinforces the notion that subsidies were never considered legitimate. As the number of cases filed accelerated, however, the high acceptance rate undercut America's commitment to the further liberalization of world commerce. The waiver provision, used for over 90% of the products which had technically qualified for aid, was an executive-centered mechanism for circumventing duty increases. After the right of waiver elapsed, the acceptance rate mirrored what it would have been without executive privilege. After 1979, the inclusion of an injury test in all CD cases assured that the pre-1974 acceptance rates of over 90% would not recur.

Perhaps most striking about the anti-dumping duty data is its long history of use. The anti-dumping law has been used consistently since the early 1960s as a mechanism to keep the American market "fair." Low approval rates in the late 1950s may be attributed to American economic preponderance—few foreign manufacturers had yet acquired the capacity to endanger American producers at home. With postwar recovery, anti-dumping legislation again became a central element of American trade policy. From an acceptance rate of under 5% in the early 1960s, dumping duties increased to an average 21% after 1963.

Instead of societal pressure as measured by petition activity, changes in legal criteria through congressional activity may be an explanation for changes in the acceptance rate of dumping cases. Petition activity decreased under the 1962 Trade Expansion Act, and increased dramatically under the 1974 Trade Reform bill. The acceptance rate went in a somewhat different direction. The years covered by the 1962 act were years of high acceptance rates for petitions (22%) relative to the periods covered by the 1958 and the 1974 acts. This increase occurred mainly in the post-1971 period when both the number of petitions and their acceptance rates accelerated rather precipitously. After 1979, acceptance rates went back up to that level found in the late 1960s. Between 1979 and 1982, 25% of all petitions were successful. These changes are not directly due to shifts in legal criteria. For instance, although low rates of acceptance are found under the 1974 law, most commentators, as well as America's trading partners, argued that the 1974 law made dumping receipt easier, not harder. However, low rates under the 1974 act may be due to the use of the trigger price mechanism to "pull back" steel cases. After 1979 and the shift of administration to Commerce, rates did increase, but to pin this increase to the change in administration would be premature. The Commerce Department is accepting petitions at about the same rate as did Treasury in the 1960s.

The cases of unfair trade or Section 337s complete the troika of cases examined which quell market disruptions. The procedure for obtaining a market exclusion as a result of a claim under Section 337 is the most technical of the types of protectionism examined. Section 337 cases typically involve complex issues relating to patent law and/or antitrust violation. Adjudication is conducted by one or two ITC administrative law judges who conduct hearings and make recommendations to the commission as a whole. The procedure encourages settlement between parties. Overall, in the period studied there was a greater number of settlements (49) than positive findings (35). Further, although there were other reasons why an ITC ruling could be overruled (most often due to action in the courts), the president intervened in almost none of the cases, although so empowered. Two explanations can be offered. First, in most cases a positive finding clearly represents an international predatory practice on the part of a particular foreign producer to "steal" American technology. The laws judged to be violated not only relate to international norms, but to domestic law enforced by the Federal Trade Commission. Patent violations or other forms of industrial espionage are not sanctioned by any international norm. Any nation that finds such violations is expected to react harshly. Secondly, a positive ruling arises from a complex process. It is difficult to argue with the finding of the ITC technicians. Thus it might be said that administrative clout is here contained in the sophistication of the procedures used to adjudicate the cases. In general, both the content of the ruling and the method of its determination may serve to dampen the incidence of a presidential veto. The nature of the cases may explain why settlement is more likely and presidential action less so.

The average rate of acceptance in the period from 1958 to 1982 for Section 337s has been 16%. That rate, however, has varied from none in the late 1950s and early 1960s to 28% in the later 1970s. The number of petitions has increased dramatically since the early period, as America's technological advantage has been undercut by foreign reproduction of American products. But, as these problems have arisen, so too have solutions. High settlement rates have undercut the government's need to exclude many of these products as private parties have been encouraged to arrive at some form of agreement.

Comparatively, of the three types of unfair trade legislation, more anti-dumping petitions were filed than either countervailing duty or Section 337 petitions. In all three, petition activity increased after 1968. Although anti-dumping petitions were more common, countervailing duty petitions had the highest success rate; Section 337s had the lowest. Of the positive rulings, most have occurred since 1968. Although the number of positive rulings have increased, so too have the number of political vetoes.

This examination leads to three general conclusions. First, in the 1960s societal interest in stopping trade penetration generally increased. In the three unfair trade types examined, only anti-dumping petitions were consistenty filed throughout the period examined. After 1968, not only did anti-dumping petitions rise in number, but aid requests under countervailing duty and Section 337, less used from 1958 to 1968, became common. Since the data presented in Table 1 indicates that the American trade balance deteriorated quickly after 1968, we can assume that the increase in petition activity basically reflects increasing import pressure on home producers. Secondly, although the majority of affirmative findings occurred after 1968, they did not occur at significantly higher rates than in the earlier period. It is correct to note an increase in the aggregate number of positive rulings. Success rates, however, are not increasing. Thus, although in raw numbers more industries are getting aid, this cannot be interpreted as an increasing proclivity of the administration to give unfair trade aid. As petitions increased in number, so too did the number of political vetoes and extra-legal settlements. Constancy, not change, better describes the adjudication of these laws.

Thirdly, there is some variation across agencies involved in unfair trade legislation. However, this variation has not changed significantly over time. Acceptance rates by the ITC tend to be higher than those of the Treasury Department or the Commerce Department. This is especially evident in anti-dumping cases where both agencies examine the same case (although under different mandates). Both agencies have increased their acceptance rates slightly over time, although after 1974 the Treasury's acceptance rate declines in both anti-dumping and countervailing duty cases. The ITC's acceptance rate also dropped slightly after 1974 for anti-dumping cases, but accelerated somewhat in that period for Section 337s. Commerce Department behavior in countervailing duty cases is below that of Treasury's tenure, and about the same as Treasury's for anti-dumping cases.

These findings relate both to structural and group theory hypotheses on American protectionist policy. As with the examination of escape clause cases, variation in policy due to either increased societal interest in aid or changes in America's trade position was not found to be a significant determinant of policy. Although aid did increase after 1968, positive rulings were stabilized by the use either of political vetoes or other extra-legal settlements. As in escape clause cases, the president has continually attempted to undercut the bureaucracy, and has succeeded. Compared to escape clause cases, unfair trade cases demonstrate a higher rate of positive findings. This higher acceptance rate exists throughout the period studied, leading to the conclusion that the type of aid, and not social pressure, is a better predictor of success rates. In short, issues of fair trade receive a better hearing by the state than do issues relating to the loss of competitiveness of an industry.

Conclusion

This essay has examined American institutions created to aid industries adversely affected by import competition. Its central finding is that these institutions have not become increasingly protectionist over time. Rather, protectionism (1) has existed in the case of unfair trade legislation over the entire period covered, and (2) did not dramatically shift in response to domestic competition. As imports have increasingly dominated the market in the U.S., industries have responded by repeatedly asking the government for aid. The state's response, however, has been guided by a desire to adhere to liberal norms to the extent possible given institutional constraints.

Three findings need to be stressed before addressing the more general problem of assigning a weight to ideas in the explanation of American policy. First, in the aggregate, few industries obtain absolution from the trend toward lowered trade barriers, and even fewer are able to increase their protection after a concession has been made. It is clear that as the U.S. moved toward increased economic openness, domestic groups and sub-units of the state apparatus have put up resistance. However, the character of that resistance has been more obvious than the extent of its success.[7] This finding seems to falsify the simple assumption that the U.S. is weak and penetrated. Increased pressures did not result in changed government policy, as theories based on a model of a penetrated American state would suggest (Katzenstein, 1977).

There may be a need, however, for a conceptual differentiation between large and small petitioning industries. Large industries are unusual on two grounds. First, they have the capacity to amass a tremendous amount of political pressure. Secondly, if granted aid, large industries represent a substantial deviation from America's liberal position. Analytically, two paths to protectionism may exist (Goldstein, 1983).[8] This research suggests that the "low" path, the road for the smaller producer, is one in which symbolic receipt of aid has less of a role. Technical merit plays a greater role in the case decision. Conversely, on the "high" path, where there is a greater display of political pressure, there is a greater need for a creative response from government leaders. For example, allowing a positive ruling for the steel industry on its dumping petitions was unacceptable. In its place, some other remedies such as OMAs or VERs were offered. Although clearly not optimal, since these agreements do represent deviations from liberal policy, they are not only much less exclusive than the alternatives facing central decision makers, but are based on cooperative agreements with America's trading partners.

Secondly, certain conclusions may be reached about the policy process associated with commercial policy. In a classic analysis of tariff legislation, Lowi suggested a relationship between issue areas and policy-making processes (Lowi, 1964). Tariffs in the early part of this century were stated to be a public good amenable to pork-barrel distribution; all petitioners received some aid from Congress. In the postwar period, tariffs shifted issue areas, becoming regulatory goods and involving coalitions dictating who would receive protection. However, the data presented here suggest that tariffs are neither a distributive nor a regulatory good. Even though the constellation of interests changed in the postwar period, there was no associated change in the amount or manner of distribution of protection. Those affected by market penetration were not ignored; rather, they were given some compensation, but not in the form or amount demanded. The state does respond to well-organized social groups, but it develops the selection of goods and the criteria used to distribute benefits. In the state-created myriad of goods to be given to import-sensitive industries, effective protection was excluded. Those industries affected by cheating in the home market, however, fared quite well throughout the postwar period. However, both executive intervention and, after 1979, new GATT codes serve to undercut even these numbers of positive aid receipts. This variability cannot be explained by changing societal forces or by market penetration alone.

Thirdly, in looking at the data on adjustment assistance and escape clause cases, we find support for the position that a free trade bias exists. This bias translates into noninterference into the private sector's ability to market its products; as opposed to acceptance of domestic Keynesian economic policy, the state follows a laissez-faire approach to trade policy. Liberal states, however, cannot ignore domestic constituencies. In the U.S., the political marketplace dictates that the state "pay off" groups in some manner. Compensating industries and workers either directly or through loans may be understood as an accepted way for the state to insure internal support. Just as nineteenth and early twentieth century industry received pork-barrel preferential tariffs from Congress, mid-twentieth century industry receives transfer payments from the executive. The difference is that tariff manipulation is no longer an option with the acceptance of free trade.

The American executive, however, is not omnipotent. Rather, it is constrained by Congress, by the bureaucracy, and even by the tariff structure set in 1929. An examination of the legislative history of the trade agreements program shows that elements of both economic openness and protectionism can be found. Opposition to executive-centered trade liberalization was present from the beginning of the program in 1934, and reappeared each time legislation was required for further negotiation. In each case the president, appealing on national interest grounds, was successful in obtaining negotiating authority. In return, Congress checked presidential discretion by widening the scope of the trade-related bureaucracy. Congress reacted to constituent pleas for protection by easing the legal criteria used by the bureaucracy to redress import injury. Instead of intervention on behalf of a specific industry, Congress would ask the bureaucracy to adjudicate the case.

The position of the executive is the least ambiguous in this period. When confronted by a choice between giving aid or not, the executive gave no aid. When protectionism was mandated by the bureaucracy, the president often chose to give a transfer payment, to give less than recommended or, in the case of countervailing duties, to sanction a tariff waiver. In dumping findings, legislation leaves no recourse but to assess a duty. However, every effort was made to convince the exporter to halt the practice. This perspective suggests that the executive-centered program of a trigger price mechanism for steel was a preemptive attempt to avoid an inevitable rash of dumping findings that the industry could legally gain.

We may now return to the problem posed at the beginning of this essay. In essence, it has been argued that continued support for the liberal economic regime is a function of the acceptance by the policy-making community of a set of rules and norms. These rules and norms were derived from both historical experience and institutional constraints. They are not "liberal" as articulated in nineteenth century British thought, but are a hybrid of ideas and institutions best characterized as defending "free" and "fair" trade. This hybrid is uniquely American, differentiating U.S. policy from that of our trading partners.[9]

The defense for an independent role in policy analysis for ideas does not rule out the importance of more traditional explanations for American policy. Rather, the argument made here is that an explanation of trade policy without inclusion of this variable will be incomplete. Assigning weight to the role of ideas as compared to other variables is a task still ahead of the social sciences (see work of Odell, 1982, and Blank, 1977). Certainly, in the case presented here, both legislative and executive behavior were as influenced by the association between protectionism and the Great Depression as they were by constituent pressures. At minimum, acceptance of liberal trade policy has meant that state policy has been less protectionist than would otherwise be predicted.

Finally, ideas may be argued to have a life cycle. Confronted by sufficient environmental feedback, in this case from international structural shifts in economic relations, ideas do change. Further, although America's international interest in promoting "free" trade did not conflict with domestic and institutional support for "fair" trade until the late 1960s, this incompatibility leads to conflict which will contribute to the "aging" of liberal ideas. Will the U.S. continue its free trade orientation in the 1980s and 1990s? Probably not, but the sufficient expanation for change will not be that there was a lag between structural shifts in the international community and changes in

American policy, nor that a major coalition caused the state to change its policy. Rather, policy will change because the economic ramifications of openness will force state central decision makers to question their beliefs. In time the state will need to alter its decision rules so as to form a better fit with reality (see North, 1980). Certainly the institutional requisites for a protectionist America exist today as they have throughout the postwar period. But protectionism becoming a salient issue in the 1980s is not sufficient cause for policy change. Rather, economic decline must be re-connected to a cognitive conception of commercial policy, as was the case after the Great Depression, for the central orientation of tariff policy to again be examined. The type of policy alternative then chosen will be determined by the intellectual debate of the moment. Unless the American economy goes into fundamental decline, however, change will be but an incremental step away from its current liberal stance.

Appendix

Tables 2-8 are based on the following data sources:

Table 2: For data on petitions between 1963 and 1974, see *United States Tariff Commission, Annual Report,* 1963, pp. 18-22; 1964, pp. 6-9; 1965, pp. 3-4; 1966, pp. 1-2; 1968, pp. 3-5; 1970, pp. 8-13; 1972, pp. 7-12; 1973, pp. 6-12; 1974, pp. 6-11. For 1975 data, see *United States International Trade Commission, Annual Report,* 1975, pp. 7-12. Data for the years 1976 to 1981 were obtained from the data banks of the U.S. Department of Labor and the U.S. Department of Commerce.

Table 3: For data on petitions between 1958 and 1974, see *United States Tariff Commission, Annual Report,* 1958, pp. 3-8, 1959, pp. 3-9; 1960, pp. 6-10; 1961, pp. 5-12; 1962, pp. 4-10; 1963, pp. 4-10; 1964, pp. 4-6; 1965, pp. 1-2; 1967, p. 2; 1968, pp. 1-3; 1969, pp. 6-7; 1970, pp. 6-11; 1971, p. 7; 1972, p. 6; 1973, pp. 5-6; 1974, pp. 5-6. *Annual Report of the President of the United States on the Trade Agreements Program, Second,* p. 7; *Third,* pp. 10-11; *Fourth,* pp. 18-19; *Fifth,* pp. 20-21; *Sixth,* pp. 12-13; *Seventh,* pp. 16-17; *Eighth,* pp. 21-22; *Ninth,* p. 19; *Tenth,* pp. 25-26; *Eleventh,* pp. 31-32; *Twelfth,* pp. 54-56; *Thirteenth,* p. 31; *Fourteenth,* pp. 33-34; *Fifteenth,* pp. 29-30; *Fifteenth,* pp. 29-30; *Sixteenth,* p. 31; *Seventeenth,* p. 38; *Eighteenth,* p. 28. For data between 1975 and 1981, see *United States International Trade Commission, Annual Report,* 1975, p. 8; 1976, pp. 4-6; 1977, pp. 5-7; 1978, pp. 5-8; 1979, pp. 5-7; 1980, pp. 16-19; 1981, pp. 1-3. *Annual Report of the President of the United States on Trade Agreements Program, Twentieth,* pp. 38-39; *Twenty-First,* pp. 42-43; *Twenty-Second,* pp. 49-53; *Twenty-Third,* pp. 67-74; *Twenty-Fourth,* pp. 105-107; *Twenty-Fifth,* pp. 179-180; *Twenty-Sixth,* pp. 197-198. For ITC recommendations and presidential proclamations see *Federal Register,* February 2, 1957, p. 713; November 14, 1957, pp. 9043-9045; December 4, 1957, pp. 9687-9689; January 15, 1958, p. 281; February 5, 1958, p. 820; March 11, 1958, p. 1708; March 24, 1958, p. 1976; April 24, 1958, pp. 2721-2723; April 28, 1958, p. 2852; September 26, 1958, pp. 7475-7448; July 29, 1958, p. 6063; October 24, 1959, pp. 8625-27; July 6, 1960, p. 6340; August 29, 1960, p. 8278-79; May 20, 1961, p. 4411; August 8, 1961, p. 7113; March 26, 1962, pp. 2789-2790; March 27, 1962, pp. 2791-92; December 31, 1969, pp. 2045-52; January 3, 1970, p. 131; February 25, 1970, pp. 3645-47; March 3, 1970, pp. 3975-3977; April 26, 1972, p. 8371; February 26, 1972, p. 4125; August 24, 1973, pp. 22833-34; April 1, 1974, pp. 11861-63; January 26, 1976, pp. 3786-87; April 6, 1976, p. 14600; May 4, 1976, pp. 18398-99; June 15, 1976, pp. 24103-05; February 4, 1977, p. 9065; March 24, 1977, pp. 15978-79; March 28, 1977, p. 16489; June 24, 1977, pp. 32430-39; June 27, 1977, pp. 32747-54; November 15, 1977, pp. 59037-40; February 9, 1978, pp. 5592-93; April 11, 1978, pp. 15127-29; September 12, 1978, pp. 40555-56; November 9, 1978, pp. 52294-95; November 17, 1978, pp. 53701-02; December 20, 1978, pp. 59445-47; January 8, 1979, pp. 1698-99; February 26, 1979, pp. 10973-10975; December 7, 1979, pp. 70583-87; January 18, 1980, pp. 3561-63; August 27, 1980, pp. 57221-27; November 3, 1980, pp. 27217-18; October 7, 1981, pp. 49683-87; November 6,

1981, p. 55163; November 17, 1981, pp. 56407-08; December 3, 1981, pp. 62339-41; February 24, 1982, p. 7997. U.S. Congress, House, January 2, 1980. *H. Doc. 96-245*, pp. 1-2.

Table 4: For data on petitions between 1958 and 1974, see *United States Tariff Commission Annual Report*, 1958, pp. 30-32; 1959, pp. 30-33; 1960, pp. 33-35; 1961, pp. 35-36; 1962, pp. 30-32; 1963, pp. 26-29; 1964, p. 15; 1965, p. 6; 1966, pp. 6-7; 1967, pp. 9-10; 1968, p. 11; 1969, pp. 15-18; 1970, pp. 27-30; 1971, p. 14; 1972, pp. 14-16; 1973, pp. 13-14; 1974, pp. 12-14. For data between 1974 and 1982, see *United States International Trade Commission*, 1975, p. 15; 1976, pp. 7-9; 1977, pp. 9-10; 1978, pp. 11-13; 1979, pp. 10-13; 1980, pp. 24-30; 1981, pp. 10-12; 1982, pp. 11-13.

Tables 5 and 6: Data were obtained for the years 1958 to 1982 from two sources. Petitions were collected from the *Federal Register, 1958-1982*, for both Countervailing Duty and Antidumping Cases. Once a positive ruling was made by the Treasury Department, data on USITC (old Tariff Commission) rulings were found for the years 1958 to 1974, see *United States Tariff Commission, Annual Report*, 1958, pp. 32-34; 1959, pp. 33-35; 1960, pp. 35-39; 1961, pp. 36-47; 1962, pp. 32-34; 1963, pp. 29-32; 1964, pp. 15-19; 1965, pp. 6-8; 1966, pp. 10-11; 1967, pp. 11-15; 1968, pp. 11-12; 1969, pp. 19-22; 1970, pp. 30-36; 1971, pp. 16-17; 1972, pp. 16-18; 1973, pp. 16-17; 1974, pp. 16-17. For years 1975 to 1982, see *United States International Trade Commission, Annual Report*, 1975, pp. 15-16; 1976, pp. 6-7 and 10-12; 1977, pp. 11-13; 1978, pp. 10 and 16-19; 1979, pp. 8-10 and 14-17; 1980, pp. 20-23 and 30-37; 1981, pp. 4-7; 1982, pp. 5-9.

Tables 7 and 8: For data on petitions see note about Table 3 above. Data on changes in market share were computed from *U.S. Imports* FT 210 (U.S. Department of Commerce), 1963, 1967, 1972, 1977, and the *Census of Manufactures* (U.S. Department of Commerce), 1964, 1967, 1972, 1977. Product categories were those designated in the *U.S. Standard Industrial Classification Manual.*

Notes

This essay was originally prepared for the Conference on the Politics of Economic Policy, Stanford University, May 11-12, 1984. I have benefited enormously from comments on this and earlier drafts from the following individuals: Stephen Krasner, David Yoffie, Robert Keohane, Arthur Stein, Terry Moe, Roger Noll, Paul Sniderman, John Ferejohn, Matt McCubbins, Mellissa Collie, Marla Davison, and Bill Lowry.

1. These two approaches are not the only ones that could be offered to explain policy. In the more general field of foreign economic policy different authors suggest different typologies for arranging theoretical explanations. For example, Cohen (1981) presents seven modes of economic decision making; Odell (1982) examines five models, and Pastor (1980) analyzes a different five. (The construction of my own analysis was heavily influenced by Odell's "ideas" model and by Pastor's Executive-Legislative Politics model.) There are a myriad of ways to organize competing explanations. The two approaches used here are the two general types most frequently offered to explain change in an existing level of protection.

2. These remarks need two clarifications. First, the president has given periodic import relief throughout the postwar years. My claim is not that the U.S. never aids industries, but rather that these theories over-predict the amount of that aid. Second, OMAs and VERs are examples of trade agreements among nations that may not necessarily constitute protectionism as traditionally defined. As cooperative agreements over market share, OMAs are often the most liberal of a range of possible options facing an American executive.

3. The requisites for escape clause relief have varied with time. Under the 1951 law a domestic industry could petition the Tariff Commission which had one year to determine whether "increased imports had caused or threatened to cause serious injury." If the president rejected Tariff Commission advice, he merely needed to report the reason to Congress. (In 1958, Congress was given the ability to override presidential action by a two-thirds vote.)

The 1962 Trade Reform Act tightened escape clause criteria. Imports no longer merely had to "contribute substantially" to the industry's problem but had to be shown to be a "major factor." Causation had to be established between a trade concession and an absolute, and not relative rise in imports. The 1974 act returned to the pre-1962 standards. Imports again needed to only be a "major part" of industry malaise. The causal connection of increased imports to trade concessions was abolished. Congress could now overrule the president by a majority vote, but the president retained discretion over accepting ITC recommendations. After 1962, the president could award an industry receiving an affirmative finding with adjustment assistance.

4. If liberal ideology is posited to have a gestation period, protectionism would have a higher likeli-

hood of being found in the later time periods.

5. Presidential remedies were coded into three categories: Agree with ITC; Agree with part of ITC remedy; Agree with none of ITC remedy. Table 3 reflects just the first of these three categories. If both the first and second are counted, the president agreed with the ITC at a rate of 9%, 1950–62; 9%, 1963–74; 8%, 1975–78; 16%, 1979–81.

6. Escape clause cases are here organized by the eight-digit standard industrial classification (SIC) codes covered by the decision. The number of cases and products may not match, since one decision could cover a number of SIC categories.

7. Since 1980 the overvalued dollar has been repeatedly blamed for both America's negative trade balance and the rise in protectionist pressures. However, for most of the post-1958 period, with the exception of 1973–1979, the dollar has been overvalued. Since an American trade deficit existed with and without an overvalued dollar, and the great expansion of protectionist interest occurs in the years of an undervalued dollar, this variable is at best one among many. Although clearly not helping America's trading position, there is no simple relationship between the dollar and official trade policy.

8. This point is clarified in Goldstein (1983). Three findings in that work are relevant. First, no sector was highly successful in all aid categories. Electrical equipment and machinery ranked first and second for escape clause cases; textiles and food and kindred products were first and second for unfair trade cases (primary metal industries were third); and food and kindred products and leather and leather products were first and second for adjustment assistance relief. In short, there was no privileged business sector. Secondly, econometric models of who gets aid in each category showed size to be unrelated to unfair trade cases, positively related to adjustment assistance, and negatively related to escape clause cases. (Size was measured by a number of variables including shipments and employment figures.) Who applies, however, does seem to be affected by this size variable. Thirdly, special solutions to import problems have been rendered to a discrete number of industries. Steel, autos, textiles, color television sets, and footwear are the best known, although world quotas have been set on a range of other items from clothespins to sugar. At the same time, producers of no great political weight have received substantial tariff or quota relief, such as the mushroom, citizens' band (CB) radio and motorcycle industries. The case-by-case data reveal no preference to give the better known industries aid; the aid granted to the CB radio industry was certainly more exclusive than that ever given to steel. The high road industries do have a greater probability of gaining political attention, but because of their weight in international trade will probably get a less exclusive agreement than could a less significant industry.

9. Although the U.S. has a unique set of ideological preferences on trade, we share subsets of these beliefs with others. The U.S. and Germany in the recent period and Britain in the nineteenth century, sharing a belief in free trade, hold a closer position on trade matters than the U.S. currently holds with either France or Japan. Variation between countries with similar ideological preferences is the result of differing historical experiences and thus institutional structures.

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POLITICS, PRIMORDIALISM, AND ORIENTALISM: MARX, ARISTOTLE, AND THE MYTH OF THE GEMEINSCHAFT

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Nineteenth-century evolutionary historical schemas formulated in Gemeinschaft-Gesellschaft, *or status to contract terms, which underpin theories of modernization and development in the contemporary social sciences, are now called into serious question. Recent archaeological discoveries show ancient society to have been preeminently contractual; and anthropological studies of family, clan, and tribe reveal them to be more than primordial relics in modernizing systems. A careful reading of the ancient writers—particularly Aristotle, who was read as lending support to the* Gemeinschaft-Gesellschaft *distinction—yields a very different picture, as Marx in fact appreciated. Family, clan, tribe, patron-client, friendship, and other affinal sets were constitutive of the ancient society of the polis, and have shown extraordinary durability in the modern political society of the Mediterranean basin. In the small-scale, urban, entrepreneurial, and commercial society of the Mediterranean polis, ancient and modern, which was characterized by a high degree of face-to-face interaction, people learned participation in the plethora of little-incorporated societies—familial, religious, cultic, and recreational.*

Much of Western thinking about politics from the early nineteenth century on has been conducted in terms of a basic three-phase schematization that meets with wide general agreement. The evolution of Western social forms and institutions is seen as a transition from (1) initially primordial society, characterized by rule of family, clan, and tribe, to (2) political society, marked by emergence of the city-state, and then to (3) advanced industrial society based upon the modern nation-state. On reflection, one notes a quite general acceptance of this schema from the inception of the modern social sciences in the nineteenth century, and virtually across their entire range. The work of nineteenth-century anthropologists in uncovering hitherto undiscovered tribal and primordial societies in Africa and Melanesia encouraged them to draw the lines more firmly between the political and the primordial, and vast numbers of social scientists have since become interested in promoting strategies of "development" to

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH, 1986

assist "underdeveloped" societies that fall within the Western sphere of influence in order to make the move to modernization.[1]

The canon for theories of modernization was set, as we know, by Max Weber's account of the rationalization of authority, the differentiation of bureaucratic structures and institutions, and the mobilization of resources that define this process. Weber, following Marx, gave the distinction between *homo politicus* and *homo oeconomicus* definition in a body of theory that accounts for the transition from the second to the third stages of the schema, or from political to modern industrial society.[2] But the theories of Marx and Weber, like those of their long line of predecessors who reflected on the origins of civil society (Hobbes, Locke, Rousseau, Adam Smith, Adam Ferguson, and others) were predicated on certain common assumptions about the transition from primordial to political society, or about the transition between the first two phases of our schema. This transition received considerable refinement in the hands of nineteenth-century thinkers with whom Weber was more directly associated: Lewis Henry Morgan, who in his *Ancient Society* (1877) characterized the transition as a shift from *societas* to *civitas*; Henry Sumner Maine, who in *The Ancient Law* (1861) characterized it as a shift from status to contract; and Weber's associate in the *Verein für Sozialpolitik*, Ferdinand Tönnies, who characterized it as a shift from *Gemeinschaft* to *Gesellschaft*, in the book of that title (1955).

There is, however, much confusion among these thinkers about what transition they are in fact explaining. Are they really describing the shift from prepolitical to political society, or are they describing, rather, the emergence of the modern industrial state that has claimed (mistakenly, as I shall try to show) exclusive rights to the mantle of politics in its classical form? This perpetual ambivalence is obvious in Tönnies' distinction between community and association, and it is even evident in Marx. Marx, Tönnies, and also Morgan, see the community, *Gemeinschaft*, as the ideal form, of which the characteristically modern form of association, *Gesellschaft*—exemplified by bourgeois, and subsequently mass, industrial society—is a corruption. However, they do not see primordial society, where property was held in common among the tribal orders, to have been the perfect form of community. It is rather the ancient commune of the polis, already admitting private property, patronage, and class politics, that they consider to have been the most perfect communal form of the past, from which modern mass society has deviated.

The matter is further confused by the fact that the study of ancient history itself fell under the shadow of Maine, Morgan, and their followers (from which it is only beginning to emerge) so that there were conscious—and less conscious—attempts to force the transition from Homeric to polis society into the status-to-contract mold. Other scholars saw in the tribal institutions of classical Greece and Rome evidence of primordialism eventually sloughed off as the political state consolidated.[3]

Developments in recent decades have conspired to cast doubt on the tripartite historical schema of primordial to political to modern industrial society. Not least among these have been the dramatic archaeological finds of the past century, which have turned up whole libraries of public and private documents and literary and religious works that give a detailed account of daily life in ancient societies for which, a hundred years ago, our only sources were Herodotus, second-hand reports of other classical writers, and the Old Testament.[4]

A second factor suggesting a reappraisal of the conventional macrohistorical schema is the recent analysis of tribal society, particularly in the Mediterranean basin, suggesting that a dichotomy between tribal/primordial compared with political (i.e., polis-based) society is unfounded. Indeed, in the case of Mediterranean and Near Eastern societies one can argue the opposite case: that for the most part there is in fact an identification between tribal and political society that demonstrates a continuity from ancient Greek and Semitic cultures through classical Rome and the modern Middle East. This does not rule out the possibility that a distinction is warranted between the primitive tribal societies of, for instance, Africa and Melanesia, and the "political" (polis-based) societies of the Mediterranean and Near East, but suggests, rather, that tribalism is itself a heterogeneous phenomenon, and that perhaps the more apt distinction is between urban/centralized and rural/nomadic decentralized societies.

A third factor suggesting the inappropriateness of the tripartite historical schema is recent work by comparative anthropologists, sociologists, and political scientists. Their efforts encompass network systems, familism, patron-client relations, and other affective, affinal, and traditional social structures and systems of power which exist alongside modern "rational" institutions, or for which in many cases apparently rational bureaucratic structures are surrogates. The persistence of familial and traditional associational groups—or their modern substitutes in the form of tribal and kinship networks, spiritual godparents, and the peculiar role of religious and sporting clubs and professional syndicates in Greece, Italy, and the Middle East—warrants serious consideration as more than primordial relics in modernizing systems.

The tripartite evolutionary schema that spells out the transition from primordial society to the classical polis, and from the city-state to the modern industrial nation-state as the *cursus honorum* of social development, was first called into question seriously by Weber. He rejected as tactfully as possible the *Gemeinschaft-Gesellschaft* distinction of his associate Ferdinand Tönnies, although he accepted Maine's concept of a shift from status to contract. Generally speaking, Weber was disinclined to accept evolutionary historical schematizations, which put distance between him and Marx—who was a convinced evolutionist even to the point of embracing the theories of Charles Darwin as an historical model. But Weber's rejection of evolutionary models, partly due to a certain ethnocentrism that posed a cultural blinder, was not radical enough. He still tended to see the significant landmarks between different forms of society as watersheds in the history of Germanic culture—witness the emphasis on the *Burgertum* as the vehicle of transition from traditional to modern society (Weber, 1958).

A more radical revision of the conventional historical schema, and the one proposed here, would see the differences between the societies of *homo politicus* and *homo oeconomicus*, that is to say, between "traditional" and modern industrial society, not as chronological or developmental, but as primarily regional differences. The societies of Greece, Italy, and the Near East have been political from time immemorial, by which is meant they are urban, polis-based, and characterized by tribal, family, cultic, religious, and occupational institutions as networks of political power. The *politeia*, or republic, is thus a more or less successful aggregate of the little societies that constitute it which enjoy a life of their own and a fair degree of autonomy. Evidence that we now have for the ancient city from second millenium Babylonian sites of the period of the Hammurabi Code points to a composite entity within which tribal groupings, the court and its bureaucracy, and the rentier class of urban notables—a "closely intermarrying pool of judges, merchants and scribes" (Adams, 1969, p. 189)—had their separate locales. Foreign traders were located outside the city at the harbour, and enjoyed an "autarchic economic" climate (Oppenheim, 1969, p. 6) now considered typical of Near Eastern society. Documentary evidence, in the form of innumerable private contracts witnessed by city and royal officials, as well as by priests, professional men, and other citizens of high standing listed according to rank, suggests a fair degree of autonomy for the city as a corporate entity, such that it could buy and sell real estate, decide legal cases, petition the court, and be held accountable for its actions. Other features reminiscent of the classical polis of antiquity and its legacy in the Islamic cities of the Mediterranean region are the election of eponymous officials for a one-year term, allowing the rotation of office among a small group of isonomous peers; the cooption of wealthy citizens to privately underwrite costly public functions (reminiscent of the Greek liturgy in later times); and the commercial development of agriculture by a slave-owning rentier class, with control of the hinterland from the city (Oppenheim, 1969, pp. 9–15). These are, indeed, the oldest urban, entrepreneurial, and commercial societies for which we have records, and their social structure has shown a surprising durability over some five millennia. Thus we may fairly characterize them as the habitat of *homo politicus.*

By contrast, Northern Europe, as it emerged from the decentralized rural communities of the Germanic and Celtic tribes, constitutes, along with its colonial empires, the locus of *homo oeconomicus.* Characterized by a separation from earliest times of military, political, and economic functions, this was a form of society which gave no real experience of political power to the majority of the population. Weber, in his comparison of Mediterranean and oriental cities with those of medieval Europe, pointed out that whereas the former enjoyed relatively autonomous existences as fortresses, market centers, and viable social communities, the medieval cities of northern Europe lacked autonomy, or enjoyed what autonomy they had as a dispensation of nobles with a monopoly of coercive force who lived in the countryside (Weber, 1968, vol. 3, ch. 16). Only in the relatively densely populated, heterogeneous, and contiguous community of the city was competition for power possible, in which family and tribal structures, cultic, recreational, and occupational forms of association played such an important role. In their typical form as scattered and isolated household units, the familial structures of the Germanic and Celtic tribes were unserviceable as political vehicles. The cities of medieval Northern Europe arose not as *poleis,* but as extensions of the essentially feudal society out of which they emerged, in which the only public person was the king. The revival of Roman Law in the Celtic and Germanic cities saw, paradoxically enough, the transfer to the king of the ancient rights of corporate immunity, rights to property, etc. once enjoyed by the cultic *collegia,* the *universitates,* and other forms of popular society. This process reached its apogee with English theories of sovereignty, in which the King is defined as a "corporation sole," enjoying a public right to sue and be sued while possessing private and personal immunity under the principal of "the king can do no wrong" (which remains the basis of municipal immunity in the tort systems of English and American common law). Medieval and early modern theories of sovereignty, which laid the basis for the absolute monarchies of northern Europe, typically applied Roman law—specifically corporation theory, which had been initially developed to define the rights of subordinate groups vis-à-vis the imperial power —in this manner.[5]

The great mystery that has preoccupied macrohistorians since Marx, and Weber in particular—why capitalism, characterized by *homo oeconomicus,* developed in the West and not in the East, despite the ability of the ancient monetarized economies of Sumeria, Babylon, Pharaonic Egypt, and the Greek and Roman Empires to develop wealth on a large scale—may be explained negatively in the following way. The rise of capitalism in the West may be attributed to a number of interrelated factors: the general absence of indigenous political structures capable of obstructing or competing for the right to economic accumulation on the part of a few; the role of the monarchies in granting royal patents and commercial concessions, as well as land, on grace and favour terms; the complete lack of the experience of political power on the part of the majority, who merely shifted status from indentured serf to that of wage-labourer; and the absence of indigenous familial, tribal, cultic, and occupational clubs with relative autonomy which—in the urban, entrepreneurial, and political societies of the Mediterranean and Orient—have enjoyed intermittently the experience of power for millennia.

Not least among the factors differentiating the society of *homo politicus* from that of *homo oeconomicus* so that capitalism as we know it was peculiar to the latter, were the religious prohibitions against the lending of capital at interest (specifically among coreligionists, but also more generally) perpetuated from their pagan predecessors by Judaism, Christianity, and Islam. Indicative of the aspirations of the autocephalous communities of ancient and oriental cities to preserve their time-honoured democratic and fraternal spirit against large-scale monopolists, these prohibitions led to the perpetuation of ancient forms of association for business purposes. The economic imperative within a citizenry dominated by merchants to keep one's capital circulating at all times while spreading the risk as widely as possible (Goitein, 1967–1983, vol. 1, pp. 154–5, 203, 263) was pursued through the instrumentality of partnerships of various kinds, and the *commenda*, or limited joint venture. For these purposes, friendship became formalized as an association of trust that permitted economic diversification beyond the traditional economic units of family, clan, and religious confession. Indeed, as the superb collection of medieval documents, sacred, secular, and communal, for the Jewish communities of the Islamic world serves to demonstrate, "friendships," as the foundation of economic activity, and partnerships were as common between confessional communities as within them.

Partnerships happily accommodated the mutual interests of the investor, who supplied capital to the venture in exchange for labour, and the nonpropertied partner, who participated as an equal, thereby avoiding the dreaded employer-employee relation too easily mistaken for that of master and slave (and these were all slave societies). Among the plethora of forms of contractual association, partnerships in the form of the mutual loan (capital and labour), the *commenda,* as the European counterpart of this Islamic form of partnership came to be known (much discussed by Max Weber as of economic significance in the development of the Italian city states), were most nicely tailored to the needs of a society of urban notables who must distance themselves from "work" and tread the fine line between investment of wealth and profits from capital (Weber, 1968, vol. 3, chap. 16, pp. 1216, 1294–95).

An economy based on networks of voluntary cooperation expressed in partnerships, commendas, and family trusts, fitted the exigencies of social systems where economic enterprise was not considered the normal business of the state, and where economic intervention was limited to sporadic revenue raising and direct buying (Goitein, 1967-1983, vol. 1, pp. 66, 266–72). The legal and contractual form that these partnerships took followed wider patterns of contractual relations in entrepreneurial communities in which "freedom of contract" was sacrosanct, and where all contracts, including marriage, were individually tailored documents rather than ritualistic symbols. For instance, marriage contracts would enumerate the material goods and chattels brought to the arrangement, as well as the expectations and desires of the future partners (some including stipulations that the husband not remove his wife to the provinces without her express permission [Goitein, 1969, pp. 83–84]) and provisions for divorce and demise, inheritance, the rights of the wife in the case of subsequent marriages, etc. In the same way commercial partnerships specified in detail the number and status of the partners to the contract; the objects of the contract (partners were not above stating as an express aim the desire to benefit from the greater prestige of one's associate [Goitein, 1967–1983, vol. 1, p. 174]); the nature and extent of the contribution that each partner made (capital, goods, labour, premises, etc.); the partners' share in profit and loss and conditions governing expenditure and disbursement; conflict of interest and whether or not the partners could enter other ventures in the same field; the duration of the partnership—normally for a year unless renewed; the accounting period, and so on (Goitein, 1967–1983, vol. 1, pp. 171–2).

Although apparently individualistic and potentially unstable, these individually contracted arrangements worked well in the relatively small-scale society of the city, where the reference group was finite and members interacted personally. The individual freedom thus afforded and the scope for individual enterprise, long masked by stereotypes of "oriental despotism" and "underdevelopment" have only recently been publicized (Goitein, 1967–1983, vols. 1, 2, 3; Goitein, 1969; Lapidus, 1967, 1969; Lewis, 1937; Morony, 1984). As Paul Veyne (1976) has demonstrated in his superb work on the ancient economy, a high level of regional autonomy was not inconsistent with a strong imperial power, for the ancient and oriental imperiums controlled their subjects at a distance, through the agency of the tax farmer, police, and the smattering of bureaucratic officials, who were usually, if not always, content to allow indigenous communities to conduct their business in peace, so long as they paid their taxes and did their military service. An indication of the low profile maintained by the imperial power is the relative absence of government buildings (Goitein, 1969, p. 90). Apart from the Mint and the Exchange, the police station, the prison, and the various offices which dispensed licenses for certain purposes, most of the public buildings and the volume of activity they represented—sugar factories, flour mills, oil presses, abattoirs, granaries, bourses, shops and bath-houses—belonged to the municipality, not the government. An equally impressive range of public buildings were the preserve of confessional communities within the city: synagogues, churches, and mosques, as well as schools, convents and monasteries, universities, law courts, and hospices. In fact, landholding by the religious communities as corporate bodies, through the "community chest" or in the form of benefices and *waqfs*, rivalled the individual and corporate holdings of the big merchants and notables.

The funding and administration of these communal organizations is a story in itself (see Veyne, 1976; Goitein, 1967–1983, vol. 2; Morony, 1984), best summed up in the concept of *évergétisme*, a neologism derived by French scholars from the term used in Greek inscriptions to honour those notables who through their wealth and good works benefited their city (*evergetein ten polin*), and corresponding to the Latin term used by Cicero, *beneficia*, rather unsatisfactorily translated into English as "services." Briefly, *évergétisme* accounts for the system of liturgies prevalent throughout the ancient and oriental worlds (and still present in the Islamic in the form of *waqfs*), whereby a floating class of notables, the political and economic elite of municipalities and their subgroups, undertook to fund public services, benefits, buildings, etc., at their own expense in exchange for public honours, prestige, and the right to rule.

The relative economic and political autonomy that such a system yielded for municipalities under the various empires of the ancient Near East—Hellenistic, Roman, Byzantine, Umayyad, Abbasid, Fatimid, Mamluk, and Ottoman—has been nicely described by Veyne in his account of conditions under the Roman Empire:

> Independent or simply autonomous in relation to the central power, the city was the ultimate frame of reference for social life just as the Empire was the ultimate framework of political life. For it was the center for decisions in matters concerning everyday life, the reference point for social distance, comparisons being made from city to city. It was in the city that the *évergètes* shone by their munificence; it was their city that they wished to dazzle at the expense of neighbouring cities, raising in it the most beautiful monuments "ad aemulationem alterius civitatis" as [Justinian's] Digest put it [50, 10, 3]. Socially, psychologically, and, not least, administratively, the city was self-sufficient: it was autarchic in Aristotle's sense, When a Roman or a Greek, subjects of the Roman Emperor, spoke of his "country" this word, *patria*, designated his city, never the Empire. There was no such thing as a Roman bourgeoisie, but a Pompeian, Athenian, or Ephesian bourgeoisie. The artisans enlisted in the professional associations were not members of an international or a syndicate: there were only local associations and one was a member of the "college" of carpenters of Lyons or the bakers of Sétif. . . . One's country was visible to the eyes, for one was able physically to gather it together in one public place (in classical Greece it even happened that the whole city literally turned out). At that time it was in the town that "everyone knew one another" as we say of the village. Such were these societies where everyone knew one another, at least by sight or by name, where everyone was engaged with the same preoccupations—and everywhere around them, thanks to the *évergètes* was an air of grand architecture. (Veyne, 1976, pp. 107-8, my translation.)

Such a romantic picture of civility is not too far from the truth in the great oriental cities where, as they became increasingly large conurbations, the frame of reference shifted from the city as such to the neighbourhood.

Marx on the Ancient and Oriental Communes

The nineteenth-century myth of the *Gemeinschaft* arises, as I have suggested, on the one hand from an ignorance of the highly contractual nature, revealed by subsequent archaeological discoveries, of the ancient oriental society that long predated Greece and Rome (its continuation and culmination in so many respects), and on the other hand from a misinterpretation of the nature and function of family, clan, and tribal structures in antiquity. A careful reading of the ancient authors—Aristotle in particular—on the relation between the *polis* (city), and the *oikos* (family) might have raised suspicions from the beginning about the efficacy of the distinction. Marx, for instance, was in no doubt that the tribes of classical Greece were inherently political, familial forms constituting integral links in the network of power-sharing groups that made up the participatory structure of the polis. He contrasted the city-state, based on the twin principles of kinship and landed property, with the decentralized, apolitical society of the Germanic tribes that, lacking any on-going communal institutions, and specifically the corporate structures of a city, remained a mere collection of isolated households. However, the idea of progress, a nineteenth-century preoccupation, stood in the way of his admitting the implications of this insight. As it turned out, Morgan later won him over rather easily to the view that classical antiquity was in effect a primordial society in which the *gens* (clan) was the most significant social form. Nevertheless, this represents a change of heart on the part of Marx, whose understanding of the political forms of classical Greece, if not those of the Orient, showed customary perspicacity.

In the *Grundrisse*, in which Marx submits communal forms, and especially those of antiquity, to their closest examination, a theory is produced with striking parallels to Aristotle's.[6] Like Aristotle, Marx has no anti-urban bias, and he refrains from glorifying the prepolitical, tribal society of the Mycenaean, or even of the Homeric age as *gemeinschaftlich* in comparison to the polis society of the classical period. Quite to the contrary, he considers the polis, with its rudimentary separation of communal and private property, and the incipient duality of individualism and communism to which it gives rise—and which is the eventual source of its destruction—to be the authentic, and perhaps the highest, form of commune among historical forms of the past. Indeed, it is a curiosity of Marx's thought that the standpoint from which he judged Western European society of his day was that of the polis and its virtues. One could even dare to suggest that the phenomenon of "alienation," which sums up that judgment, is more a product of Marx's propensity to hold *homo oeconomicus* to account by the standards of *homo politicus* than it is a reflection of fundamental dislocations within the capitalist system. The agonistic virtues or warrior excellences of the polis, and the rough democracy of the all-male *gymnasia* and political clubs which constituted it, were perhaps mirrored for Marx in the struggles between the revolutionary parties and the fraternal socialist societies and syndicates of his own political world, social forms otherwise lost in the bland and depersonalized structures of the modern state.

Marx analyzes the communal forms of the pre-modern period into three groups: (1) the oriental commune, (2) the polis type, and (3) the community of the Germanic tribes. He betrays a general ignorance of oriental society, which we now know to have included city-states of the most highly developed type, by characterizing it as exclusively nomadic. In oriental society he sees the primordial type of spontaneous clan organization that he imagines to be the archetype for all forms of primitive society, and over which the city-state structures of the polis were seen to represent a significant advance. He maintains that in oriental society, communality (*Gemeinschaftlichkeit*) is accidental, a mere function of common land ownership and ethnicity. The polis community marks a watershed in the evolution of communal forms because of the very fact of the coexistence of communal and private property.[7] Marx speculated that the classical polis evolved gradually from the clan-dominated tribal commune, for which he believed the oriental city was the model: an "initial naturally arisen spontaneous community (*naturwüchsiges Gemeinwesen*)," constituted of "the family, the family extended as a clan (*Stamm*), or through intermarriage between families, or combinations of clans" (Marx, 1953, p. 375; 1973, p. 472). Once it became settled in the form of the polis "this original community (*ursprüngliche Gemeinschaft*)" was modified by "removal of the clan from its original seat," the "occupation of alien ground," and "new conditions of labour" which permitted the individual to "become a *private proprietor* of land and soil—of a particular plot—whose particular cultivation falls to him and his family" (Marx, 1953, pp. 378–9; 1973, p. 475). Thus two factors distinguish the second type of commune, the polis, from the first type, the oriental: (1) the separation of communal and private property in the more highly developed form; and (2) the urban nature of the polis as opposed to the characteristically pastoral oriental commune. "The history of classical antiquity is the history of cities," Marx maintains, "but of cities founded on landed property and agriculture" (1953, p. 382; 1973, p. 479).

Here, of course, he was quite wrong. Far from being antithetical, all the evidence suggests a direct parallel between the ancient oriental city and the classical polis. Babylonian and Assyrian cities from the second millenium B.C. were not pastoral communities, but farmed their fields (whether attached to the municipality or outlying), as commercial ventures underwritten by rentiers, either individually or in partnerships, using risk capital, mechanization, and employing professional managers and slaves. The produce was stored in state granaries under the supervision of municipal officials, and the proceeds disbursed under an elaborate accounting system, once taxes had been collected (Oppenheim, 1969, pp. 13–15). Control of the countryside by the city has continued to be a feature of Middle Eastern society, as has the mix of public and private ownership along these lines. Goitein points out that in the medieval period, while orchards were privately owned, fields and vineyards—which were classed as fields—were legally the property of the state and its officials (Goitein, 1967–1983, vol. 1, p. 123). Even under Nasserite socialism the same division prevailed, the private ownership of orchards was often in the form of partnerships where blocks of trees were apportioned according to shares, while fields were farmed as state collectives and worked by the *fellahin.*

The classical commune, the second form in Marx's series, is distinguished from the commune of the Germanic tribes, the third, by the same characteristics that distinguished it from the oriental commune: by virtue of the separation of private and communal property and by virtue of being a city—factors which are mutually related. It is "with its coming-together in the city" that "the commune (*die Gemeinde*) possesses an economic existence as such," Marx argues: "the city's mere *presence,* as such, distinguished it from a mere multiplicity of independent houses." In other words "the whole, here, consists not merely of its parts; it is a kind of independent organism" (1953, p. 382; 1973, p. 483).

It is clear that Marx considers Germanic tribal society as such as not properly corporate in the way that the city-state was. He refers to the tribal chiefs, scattered through the Germanic forests in their patriarchal households, and their periodic assemblies as a "coming-together (*Vereinigung*) . . . not a being-together (*Verein*); as a unification made up of independent subjects, landed proprietors, and not as a unity" (1953, p. 383; 1973, p. 483). More specifically, "the commune (*die Gemeinde*) therefore does not exist as a state or political body," as in classical antiquity, because it does not "exist as a city." This means that "for the commune to come into real existence, the free landed proprietors have to hold a meeting, whereas e.g. in Rome it exists even apart from these assemblies in the existence of the city itself and of the officials presiding over it. . ." (1953, p. 383; 1973, p. 483). Tribal society, like the oriental commune, is superficially communal due to gentilitian ties and familial forms in which the unity of the clan system "is represented in a chief of the clan family, or as the relation of the patriarchs (*Familienväter*) among one another" (1953, p. 377; 1973, p. 473). Only with the polis, however, does the commune possess "an economic existence as such" (1953, p. 383; 1973, p. 483). The difference between the classical commune and that of the Germanic tribes is for all the world like that between polis and Homeric *oikos* society made up of independent, self-sufficient households (*oikoi*):

> In the world of antiquity, the city with its territory is the economic totality; in the Germanic world, the totality is the individual residence, which itself appears as only a small dot on the land belonging to it, and which is not a concentration of many proprietors, but the family as independent unit. . . . In the Germanic form, the agriculturalist [is] not [a] citizen of a state; i.e. not an inhabitant of a city; [the] basis [is] rather the isolated, independent family residence, guaranteed by the bond with other such family residences of the same tribe (*Stamm*), and by their occasional coming-together (*Zusammenkommen*) to pledge each others' allegiance in war, religion, adjudication, etc. (Marx, 1953, pp. 383–4; 1973, p. 484)

Marx believes that in the case of the Germanic tribes, like the oriental commune, initially social forms of appropriation represented by common "language, blood," and land are not translated into an incorporated community: "the commune (*die Gemeinde*) exists only in the interrelations among . . . individual landed proprietors as such; communal property (*das Gemeindeeigentum*) as such appears only as a communal accessory (*gemeinschaftliches Zubehör*) to the individual tribal seats (*Stammsitzen*) and the land they appropriate (*Bodenaneignungen*)" (1953, p. 384; 1973, p. 484).

The significance of landed property in classical antiquity under the polis form is that, although mediated by the commune, it can be owned privately, thus allowing individual self-determination within the bounds of communal interests. Reaping the advantages of a qualified individualism and greater development of human powers and capacities through private property, "the commune (*Gemeinde*)—as state [i.e. as polis]—is, on the one side, the relation of these free and equal private proprietors to one another, their bond against the outside, and is at the same time their safeguard" (Marx, 1953, pp. 378-9; 1973, p. 475).

Marx characterizes the economic activity of the classical commune in Aristotelian terms: it is properly directed toward the self-sufficiency (*autarkia*) of the community as such, and not the production of wealth. For this reason the polis as an affinal entity transcending family, clan, and tribe, which characterized oriental and Germanic societies, is superior to the spontaneous primordial forms of association that become its subunits: "it is not cooperation in *wealth-producing* labour by means of which the commune member reproduces himself, but rather cooperation in labour for communal interests (*gemeinschaftlichen Interessen*) (imaginary and real) for the upholding of the association (*des Verbandes*) inwardly and outwardly" (1953, p. 380; 1973, p. 476).

Such a highly romanticized view of public-spiritedness in the polis is hardly consistent with the facts. One is prompted to wonder why, indeed, Marx ever referred to the classical polis—already showing the signs of the tension between private and communal property that would ultimately be the source of its destruction—as the most highly developed form of commune in historical experience. But one might have posed the same question to Aristotle, aristocratic in his predilections, and in many respects still wedded to the Homeric code. Why, since he hoped to ban retail trade and curtail commerce, making no provisions for the new rich to participate in power, did he not, therefore, recommend the *oikos* society of the Homeric period—as a collection of households bound by primordial ties—over the polis, potentially divided by class and threatened by individualism and acquisitiveness?

Aristotle's response is similar to Marx's. "It is manifest," the former maintains, "that a state is not merely the sharing of a common locality for the purpose of preventing mutual injury and exchanging goods." "These are necessary preconditions of a state's existence," he notes, but "even if all these conditions are present, that does not therefore make a state (*polis*)." The city is "a partnership (*koinonia*) of families and of clans" for the purpose of "living well, and its object is a full and independent (*autarkous*) life." For this to be realized, members of the community must "inhabit one and the same locality and practice intermarriage." This, Aristotle argues, is "the reason why family relationships have arisen throughout the states, and brotherhoods and clubs for sacrificial rites and social recreation." The root of such forms of association is "the feeling of friendship (*philia*)," or love; for "friendship is the motive of social life," and "while the object of a state is the good life, these things are means to that end." In sum: "a state is the partnership of clans and villages in a full and independent life, which in our view constitutes a happy and noble life; the political fellowship (*politikon koinonian*) must therefore be deemed to exist for the sake of noble actions, not merely for living in common" (*Politics*, 3.5.13-14.1280b30-1281a15, Loeb ed., pp. 217-219).[8]

Ironically, no description could better fit the oriental city, a federation of autocephalous communities, each resident in its separate locality and governed by kinship, confessional and fraternal organizations, and by forms of mutual association through which politics and business were transacted. In ignorance of the documentary evidence now available, and due in no small part, one suspects, to a certain ethnocentrism, nineteenth-century European scholars consistently misinterpreted the oriental city and its relation to the polis. For instance, Max Weber (1968, vol. 3, chap. 16, pp. 1285–93) concedes that the Mediterranean *poleis* (which would surely feature the oriental cities) were spontaneous synoecisms of ethnically diverse communities that contracted to live peacefully together, and that maintained a rough equality among themselves, developing an increasingly democratic politics. The Northern European medieval cities, on the other hand, lived on the grace and favour of a landed nobility whose interests were served by the development of market and manufacturing towns only so long as the political aspirations of the townsmen were confined to policing their occupations through the guilds. At the same time, Weber quite arbitrarily attributes to the European medieval city a corporate identity and charter of rights and immunities absent in the oriental city (1968, vol. 3, chap. 16, p. 1220). Not only is this far from being the case—in Sippar of the second millenium B.C. a parallel structure of municipal officials vis-à-vis the court defended just such corporate rights—but it reflects the typically statist assumptions of most nineteenth-century scholars—and plenty of twentieth-century ones too. Marx himself was among them. So preoccupied are they with the centralized authority of the modern state that the past is read back systematically from the present. Thus the struggle of the Northern European *popolo* against despotic sovereign authorities is considered an historical advance over the decentralized, pluralistic, Mediterranean, and oriental communities that had already won their concessions, or perhaps never needed to. In Marx's case this view sits uneasily with his general glorification of the polis as a social form.

The extraordinarily speculative and undocumented view of oriental society which Marx and Weber represent, and which we now know to be false, has nevertheless been quite pervasive in its influence. It found its possible source in the mythology of Islam, which saw itself as a purifying force that purged the decadent "sedentary" civilizations of the Levant with the values of conquering Bedouin tribes from the hinterland. However, the self-perpetuated mythology of Islam as a tribal religious culture in its desert fastnesses was not even true in the time of Ibn Khaldun (1958), who bears a major responsibility for propagating it. Mohammed was the product of a sophisticated merchant city, and the early literature and the legal, educational, and philosophical traditions of Islam bear the stamp of a merchant class that played a formative role in its development (Cook, 1974, pp. 226–7). Orientalists have argued for some time now that the Islamic conquest gave rise to cities in which the newcomers were soon assimilated to the ways of the old, and where tribalism rapidly became a notional entity—a collective badge and a link with the past that tied together the Islamic urban elite, which displaced an older class of notables at the municipal level, and at the same time provided neighbourhoods an identity reinforced by the ties of family, clientage, common village origin, and the full range of fraternal organizations—Sufi orders, youth gangs, etc.—so reminiscent of the Athenian cults, fraternities (*phratries*), and often thuggish political clubs (*hetaireiai*) (Calhoun, 1913; Lapidus, 1969, pp. 49ff).

One can see time and again the confusion that Marx's (and Weber's) thesis about the unincorporated nature of oriental society still produces. For instance, Nadav Safran, addressing Weber's thesis directly, maintains that the reason why "the Muslim city did not develop a corporate political entity" or "strong interest groups," like those of medieval Western burgher-groups, is because cities in the oriental world, unlike those of medieval

Europe, were seats of government, and "the government as the dominant urban interest did not allow other interests to develop" (Safran, 1969, pp. 76–77). Therefore, "no independent society emerged," witnessed by the fact that "there were no autonomous laws and regulations in Muslim cities, and hence there were no corporate interest groups challenging the caliphate and forcing the development of constitutional law, as took place in the West" (Safran, 1969, pp. 76–77).

This is a veiled thesis of oriental despotism (Wittfogel, 1957). Moreover, it is implausible on its face. The struggle for a single corporate identity enshrined in a centralized body of law was unnecessary (1) because the state was only one of the salient administrative elements in these societies; (2) because government was indeed located in the city and accommodated the interests of its various communities; and (3) because these communities enjoyed severally a level of rights and immunities, economic independence, and intercommunal toleration, that is remarkable even by today's standards. Goitein and others emphasize the last point over and over for Egypt (Goitein, 1967–1983, vol. 1, pp. 66ff., 266–72; Goitein, 1969, pp. 93–95; Goitein, 1967–1983, vol. 2; Lewis, 1937; Lapidus, 1967, 1969); and it was true even for Iraq, whose cities were the seats of those successive empires of increasing magnitude and ever more tightly controlled bureaucratic apparatuses: the Babylonian, Assyrian, Achaemenid, Parthian, and Sassanian (Morony, 1984).

In a parallel kind of argument, informal forms of association, craft, and profession, as well as confessional, familial, and fraternal forms, are frequently forced into the mold of the European guild—or deemed inferior if they do not fit. However, as Goitein categorically states, at least until the fourteenth century there was no such institution, because there was no need of it: "the classic Islamic society was a free enterprise society, the very opposite of a community organized in rigid guilds and tight professional corporations" (Goitein, 1969, p. 94). Moslem corporations with characteristic rights and immunities abound, frequently adopting the ritualistic practices of the Sufi orders and brotherhoods. However, supervision of the work of artisans was not typically one of their functions, but was a duty placed "in the hands of the state police, which availed itself of the services of trustworthy and expert assistants" (Goitein, 1969, p. 94).[9]

Aristotle on the Contractual Nature of the Polis

Aristotle maintained, and Marx affirmed, that the polis better realizes the human *telos*, for man is by nature a political animal (*zoon politikon*), and this means simply that he is a city (*polis*) dweller. Only the proximity and multifaceted communal ties produced by contiguous existence permit the objectification of communal forms in everyday life, and, at the same time, the full flowering of the many-sided individual. Aristotle suggested, and recent scholars affirm, that the forms of association to which he refers —family (*oikia*), clans (*gene*), tribes (*phyle*), brotherhoods (*phratriai*), clubs (*hetaireiai*), and cultic organizations (*thysiai*)—were always political, and not primordial, as far back as the historical record goes.[10] Although in the *Laws* (680D-E) Plato gives a genetic account of the emergence of political society out of the primordial—a collection of scattered households and patriarchal clans of which Marx's Germanic tribes are reminiscent—it is difficult to separate fact from myth. Aristotle's account lends support to the idea that, although political society was organized along apparently familial lines, the link of consanguinity between the clans and tribes, into which the city was organized for administrative purposes, was more fictional than real. Moreover, quite apart from the havoc to blood lines wrought by migration, on which both Marx and Aristotle comment, the deliberate policy of reformers like Solon and Cleisthenes had been to "mix it all up" by encouraging immigration, by the enfranchisement of aliens, by realigning the class structure to reward the upwardly mobile, and by reconstituting the tribal structure on a regional basis to destroy the power base of some families and strengthen that of others, in particular the urban tribes (Aristotle, *Politics* 6.2.11.1319b20-5, Loeb trans., p. 507).

Thus any simple association of *Gemeinschaft*, as the ideal type of organic community, with primordial society, where property was exclusively communal, and of *Gesellschaft* with urban, contractual, exchange-based society falls wide of the mark. Marx, following Aristotle, located the most highly developed form of the community in the *polis*, which was geared not merely to subsistence, but to "living well." His theory of exchange borrows explicitly from Aristotle's, and it is only in his last works, the so-called *Ethnological Notebooks*, that Marx, under the influence of Morgan, changes his mind, leaning toward a primitivist account of the *Gemeinschaft* taken up by Thomson, Godelier, Terray, and others today.

In Tönnies' version of the *Gemeinschaft-Gesellschaft* theory the use-value/exchange-value distinction was a crucial factor in distinguishing between the two forms. Invoking Hegelian, and subsequently Marxist, concepts of labour as a medium for the objectification of the human subject, he saw in the *Gemeinschaft* the appropriate social setting for this working out of human powers in the external world of objects. "Where such activity [labour] serves to maintain, further, or give joy to a *Gemeinschaft*, as is the case in natural and original relationships, it can be conceived of as a function of this *Gemeinschaft*," he declares (Tönnies, 1955, p. 92). By contrast, the *Gesellschaft* is geared to commerce, and "commerce, as the skill to make profits, is the opposite of all such art" (Tönnies, 1955, p. 92).

Much has been made by economic historians and classicists of Aristotle's distinction between *oikonomia* and *chrematistike* as forerunners of the distinctions between use-value and exchange-value in Marx, and some have even given these concepts in Aristotle a reading that would suggest an early form of the *Gemeinschaft-Gesellschaft* distinction. We may summarize Aristotle's arguments briefly in order to make two different points: (1) that Aristotle's recommendations concerning economic activity have been misinterpreted as statements of fact, leading to a serious underestimate of the significance of commerce and trade in antiquity; and (2) that the application of the *Gemeinschaft-Gesellschaft* distinction to the polis fails on all counts. The tendency to characterize the polis as the *Gemeinschaft* takes no account of Aristotle's explicit argument that all communities rest on exchange—"if there were no exchange there would be no association (*koinonia*)" (*Nicomachean Ethics* 5.5.14.1133b17-18, Loeb ed., p. 287)—or of the highly contractual nature of social relationships in the ancient world, where membership in subordinate kinship and status groups (tribes, phratries, *gene*, *orgeone*)—not to speak of marriage, friendship, citizenship, adoption, and even tribal affiliation—was a closely circumscribed and explicitly regulated contractual arrangement sealed by ceremony and ritual. However, those theorists, including Marx in his last writings when he came under Morgan's influence, who see pre-polis, primordial society as the *Gemeinschaft* miss a different mark. Their application of the distinction takes no account of the degree to which so-called primordial forms were in fact political, assuming as organic, forms of social relationship that in recorded history have borne a tenuous relation to real bloodlines or systems of consanguinity with a genetic basis.

The highly contractual nature of relations in the polis could not be better demonstrated than by the fine distinctions Aristotle makes between the different forms of transaction and the principles of justice (equality or proportionality, as they are translated into legal/juridical terms) appropriate to them. They include the private contracts between parties termed "voluntary transactions," but also include relationships between "friends" in general, friendship being understood as a contractual relationship with finely graduated scales of benefits and duties, depending on the degree of the relationship. There is, indeed, an analogue between the exchange relationship and that between private citizens that emerges when disputes arise. When disagreements arise between friends, or when parties turn to a judge for rectification of damages that arise from a breach of contract, essentially the same process is involved. What has to be achieved is agreement on compensation for gain or loss that is acceptable to both parties who construe their needs from diametrically opposed positions. The result, as in the act of exchange itself, is to achieve the impossible: an essentially asymmetrical relationship is converted into a symmetrical one by an exchange in value in which the gain and loss are equalized. Aristotle in fact admits that "the terms 'loss' and 'gain' in these cases are borrowed from the operations of voluntary [economic] exchange . . . as for instance in buying and selling, and all other transactions sanctioned by law" (*Nicomachean Ethics* 5.4.13.1132b10-15, p. 279).

Aristotle on Friendships and Partnerships

Not only is Aristotle's concept of justice specifically tailored to the need for fair contracts and equity in the distribution of goods and services that an exchange-based economy produces, but even his concept of "friendship" presupposes contractual obligations that an economy based on informal association demanded. Indeed, the point of Aristotle's typology of forms of social relationship in the *Politics*—husband/wife, father/child, master/slave, householder/household—is not so much to emphasize their inequality (a fact taken for granted in the ancient world) as it is to show that the *polis*, as "the larger community," is an aggregate of subordinate forms of association, qualitatively different and irreducible, but each of which describes a schedule of goods and benefits shared and received.

In the two chapters that he devotes to friendship in the *Nicomachean Ethics* (a more extensive treatment even than that given justice), he deals systematically with the affinal groups out of which the city is constituted. "All friendship involves community or partnership (*koinonia*)," he says (*Nicomachean Ethics* 8.9.1.1159b25-30, and 8.2.1.1161b 10-15), but some relationships are definitely more contractual than others. Relationships between the members of a family, and of comradeships (*hetaireiai*) and brotherhoods (*phratriai*), he says, are "less in the nature of partnerships than are the relationships between fellow citizens (*politikai*), fellow tribesmen (*phyletikai*), shipmates, and the like; since these seem to be founded as it were on a definite compact" (*Nicomachean Ethics* 8.12.2.1161b 10-15, pp. 497-99).

All forms of friendship are partnerships or associations for the exchange of benefits, Aristotle maintains, referring under the umbrella of friendship to the whole array of diverse associations in antiquity: tribes (*phyletai*), demes (*demotai*), cultic associations (*gennetai*), religious guilds (*thiasotai*), dining clubs (*synousias*) (*Nicomachean Ethics* 8.9.5. 1160a15-25). Since it is bound to be the case in some of these associations that one or other of the parties feels it has not gotten what it contracted for, the problem then arises of rectifying the damages in a case where gain and loss are assessed from the different points of view of the two parties. Clearly Aristotle does not mean by friendship a primarily emotional relationship. These are forms of association entered on explicitly contractual terms with procedures for arbitrating disputes in which one or other party feels that the conditions of the friendship were not met.

Modern classicists such as Adkins (1960, 1963), Long (1970), Finley (1954), Ehrenberg (1943), and others have emphasized the extent to which concepts of friendship in antiquity describe quasi-juridical relations between individuals, and only secondarily as a consequence of mutual interaction, bonds of affection. They point out that in the Homeric period, and to some extent in the classical, human beings as such had only those rights that derive from relations of birth, marriage, *philotes*, etc. Outside kinship relationships all relations of mutual reliance fell under the category of "friendship."

In the absence of a strong state to guarantee personal security, such networks were essential to the autonomy and self-sufficiency of the family (*oikos*) and its members. Adkins (1963) points out, for instance, that despite an apparent distinction between friendships based on utility and those based on virtue, all three forms of friendship described by Aristotle —the hedonic, the utilitarian, and the virtuous—are in fact pragmatic. Friendship for another *agathon* (noble), on account of virtue (*arete*), and friendship for a comrade (*hetairos*) or equal, were as necessary as any other form of friendship for self-sufficiency and survival. Thus the conventional definition of friendship that we find in Plato of "helping one's friends and harming one's enemies" (*Crito* 45c5ff., *Meno* 41e2ff., *Republic* 332D) takes on significance as a formula for one's duty, or protecting one's interests and preserving the in-group against the out-group (Adkins, 1960, pp. 228ff.).

Philologically the verb *philein*, to love, the noun *philotes*, friend, and the adjective *philos*, are interesting: *philos* being translated as "dear," "beloved," in the emotive sense, or "one's own," in the quasi-juridical sense. Thus, as Adkins points out, "the Homeric *agathos* (warrior-chieftain in charge of his own *oikos*) uses *philos* of his various faculties, possessions, and fellowmen, beginning as near as possible to the man himself and working steadily outwards" (Adkins, 1963, p. 30). So intimately is the term related to concepts of properties, possessions, and rights that it is used in Homer's *Iliad* and *Odyssey*, for instance, to refer to one's own (*philos*) limbs or one's own (*phila*) clothes or bed, respectively, where to translate the term *philos* in an emotive sense with "dear" or "beloved" would be inappropriate (Adkins, 1963, p. 30).

It has been shown, indeed, that the history of concepts of friendship and affinity in the ancient world took the form of the gradual extension of concepts of kinship (*oikeiotes*) to ever larger affinal groups (Baldry, 1965, pp. 42, 142–3, 178). From *oikos*, kinship construed in its narrowest terms, was derived more abstract concepts of likeness (*oikeiosis*) that, by virtue of a Stoic preoccupation with a common human nature transcending natural boundaries, gave rise eventually to the conception of the inhabited world as the *oikoumene*, the family of man with its natural divisions of family, clan, and tribe.

The intimate connections between kinship, ownership, and love in antiquity are very likely the basis for Aristotle's bracketing of the twin sentiments "this is my own" (property) and "this I love" (affinity) in his critique of Plato's communism in the *Republic* (*Politics* 2.1.8–12.1261b15–1262a25). Aristotle points to the extreme unity of the *Republic*, which ignores the plethora of relationships based on affinity, consanguinity, and marriage that hold the city together. What would it mean under communism to call a boy "my son" when he may be shared by any number of people from two to ten thousand? Better he were "one's private nephew than one's son in the way described" (*Politics* 2.1.12.1262a10-15). Parenthood creates a special "belonging," and Aristotle leaves no doubt that his major objection to Plato's reconstitution of social and political forms in the *Republic* is Plato's substitution of an abstract and arithmetical relation of equality and solidarity under communism for the specific and historically developed forms that have a real life in the everyday Greek world. Creating community (*koinonia*) does not come from watering down subordinate groups, but rather from creating "a partnership out of a multitude through education (*paideian koine*)" (*Politics* 2.2.10.1263b35-40, p. 91). "The state is a partnership of families, living well in their clans, brotherhoods, clubs for worship and recreation" (*Politics* 3.5.14.1280b35-1281a5). And the reason why all these subordinate social forms cannot be collapsed to a unity in communism is that community is built on social solidarity or "friendliness," as Aristotle put it simply, and the experience of social solidarity, like most virtues, can only be learned by doing it (*Nicomachean Ethics* 3.5.21.1114b25-30, Loeb trans., p. 153). Participation in the plethora of small scale social groups, familial, local, cultic, and religious, that made up everyday life in ancient Greece (as they continue to do in Mediterranean society today) provided that experience. Political society is not antithetical to the clan-based, cultic community, but is a continuation of it.

Aristotle's dissertation on friendship in books eight and nine of the *Nicomachean Ethics*, treating the varous forms of friendship, the expectations attached to them, their relative durability, and how to arbitrate in cases of breakdown where expectations have not been fulfilled, is testimony to the degree to which the *polis* as a community is based on a network of little societies whose bonds are affective but whose status is quasi-juridical. The highly contractual nature of friendship and the texture of Aristotle's arguments have been missed by those commentators from the analytic school of philosophy whose ear is not attuned to historical and cultural nuance. They have also been overlooked by proponents of the *Gemeinschaft-Gesellschaft* distinction who have seen in primordial forms of kinship and friendship, relations based on "love" antithetical to the contractual relations of modern society. In general, the relation that Aristotle's analysis of friendship and the bonding virtues of love, magnanimity, honour, and justice in the *Nicomachean Ethics* (bks. 4-5, 8-9) bears to social groups in their legally constituted form in the *Politics* (bks. 1-3) has been underestimated.

Homo Politicus and *Homo Oeconomicus*

The *Gemeinschaft-Gesellschaft* distinction needs at the very least to be revised. The more appropriate distinction, hinted at by Marx, is between properly political society—small-scale, urban, highly participatory and entrepreneurial, confined in Europe now, as in ancient times, to the Mediterranean basin—and northern European society, whose ancestry is feudal, rural, and decentralized. In these latter systems (excluding northern Italian city states and the free cities of the Hansa League) participation in free and equal institutions is only a recent part of history, and absolutism, where the only public person was the king, was once the dominant mode.

The political community of antiquity was an urban phenomenon based on a coastal entrepreneurial economy generating considerable wealth, whose life-blood was an elaborate network of forms of association—familial, religious, regional, occupational, recreational, and class. The success of fifth-century democracy was in many respects a function of the habit of associating together in a multitude of little societies. Polis society is characterized by the fact that kinship and tribe have become modified constructs tailored to the religious calendar and rotation of offices of the city (Veyne, 1976; Connor, 1971; Davies, 1981). Religious cults and the monopoly of exchange for public purposes through a system of liturgies by which private citizens underwrote the expenses of financing the fleet, the army, choral and dramatic performances, games, athletic contests, and festivals, put a brake on private accumulation in the polis. Concepts of friendship, community, obligation, and justice have a high degree of specificity and describe a way of life (*politeia*), and not abstract values or duty for duty's sake. However, as against Tönnies, the relationships between members of these fellowships (*koinonia*) are not simply organic, based on love, but are properly contractual—sealed by ritual compacts, blood covenants, etc. For this reason it is possible to distinguish different degrees of relationship which, although based on the model of different degrees of consanguinity, are not necessarily based on blood relationships at all.

The mix of public and private in these relationships parallels that of family and friend. Thus demes, phratries, and so-called clans (*gene*) were semi-official organizations, but under the control of private resources. The highly influential male clubs—*hetaireiai* (political), *symposia* (drinking), and *gymnasia* (wrestling)—had equality, if not precedence, over relations through marriage that would unduly elevate the status of women, and were in many respects public in function in a society based more on "fraternal" than patriarchal social relations.[11] Only the *oikos* itself was defined as the exclusively private sphere. Aristotle's account of the social constitution of the polis (*Politics* bks. 1–2) is consistent with this analysis. For Aristotle saw the city as the aggregate forms of partnership or association (*koinonia*), some of which were based on real or fictional concepts of consanguinity, others being based on shared religious, ethnic, or cultic background. But the *koinonia politike* is a whole greater than the sum of its parts, so that to rule over "a large household and a small city" is not the same thing, but involves a difference in kind (*Politics* 1.1.2.1252a10–15, p. 3). "The partnership that comes about in the course of nature for everyday purposes, is the *oikos*," Aristotle observes, and as the extended rather than small family it includes "food-sharers," "hearthsharers," and "sharers of the mother's milk" (*homogalaktas*), none of which according to modern authorities are necessarily blood relations. "The primary partnership made up of several households for the satisfaction of . . . needs is the village" (*Politics* 1.1.6–7. 1252b10–20, p. 7), while "that partnership which is composed of several villages is the city-state," which has "at last attained the virtual self-sufficiency" which fits it for the good life (*Politics* 1.1.8.1252b10–35, pp. 7–9).

Aristotle refuses any distinction between natural, primordial forms of association and the city as a product of art or convention: "Hence every city-state exists by nature, inasmuch as the first partnerships so exist," he maintains, "for the city-state is the end of the other partnerships, and nature is an end." It is from this point of view that "the city-state is a natural growth and that man is by nature a political animal." As a consequence, "a man that is by nature and not merely by fortune citiless (*apolis*), is either low in the scale of humanity or above it (like the 'clanless [*aphretor*], lawless [*athemistos*], hearthless [*anestios*]' man reviled by Homer)," for ". . . man is a political animal (*politikon ho anthropos zoon*) in a greater manner than any bee or gregarious animal" (*Politics* 1.1.8–9. 1252b30–1253a10, pp. 9–11). (We are reminded of Marx's several asides on the Aristotelian definition *zoon politikon* properly referring to man as a city or polis dweller, as well as his famous distinction between the architect and the bee, the former a social being whose production represents the needs and purposes of a living community, the latter a creature whose gregariousness and productivity are instinctively driven.) Aristotle's various references to subordinate forms of association make it plain that he conceives these now to be units of urban administration, whatever their original function may have been. So when he speaks of voting "by tribes (*phylas*), or demes (*demous*), or brotherhoods (*phratrias*)" (*Politics* 4.12.11.1309a10–15, p. 363), these are clearly alternative juridical units. When he recommends the auditing of public funds be carried out by "brotherhoods (*phratrias*), companies (*lochous*) and tribes (*phylas*)" (*Politics* 5.7.11.1309a10–15, p. 429), he is referring to the administrative function of these forms of association.

Certain fertile remarks in the *Grundrisse* and *Capital* suggest in fact that Marx might have formed a comprehensive theory of ancient society much closer to Aristotle's—and one in which the *Gemeinschaft-Gesellschaft* distinction would not have figured—had they been developed. For instance, Marx notes both that "the history of classical antiquity is the history of cities," and that "there was in antiquity no more general institution than that of kin groups" (1973, pp. 478–9). His references to *zoon politikon* emphasize not only that man is a town dweller by nature, but also that politics as an urban phenomenon is, as Aristotle insisted, characteristically "rule of the free." By contrast, the society of the Germanic tribes, typifying Northern European society from which the capitalist nation-states ultimately developed, is rural, decentralized, and tribal in a different sense:

> The Middle Ages (Germanic period) begins with the land as the seat of history, whose further development then moves forward in the contradiction between town and countryside; the modern age is the urbanization of the countryside, not the ruralization of the city as in antiquity. (Marx, 1973, p. 479)

Marx is hinting, perhaps, that the primordial/political distinction is a regional rather than a chronological one, and that while "political" societies (literally *poleis*) of the Mediterranean basin have never changed essentially in character, northern European society is an extension of primordialism which grew out of Franco-Germanic tribal society and its feudal legacy.

The *Gemeinschaft-Gesellschaft* distinction postulates an original primordial community ruled by family, clan, and tribe, out of which political society emerges. This proposition has been seriously called into question by modern classical scholars, who show that concepts of *genos* and clan are reconstructions of nineteenth-century scholars drawing on contemporary anthropological conceptions of tribalism in Africa, Melanesia, and Australia that are inappropriate to urban Mediterranean society in antiquity.[12] Marx fell under the influence of these nineteenth-century scholars —Morgan, Maine, Fustel de Coulanges, etc.—in his last years. So while his account of the polis in relation to other communal forms in the *Grundrisse* is much closer to the appraisal of Aristotle and those modern scholars who have turned back to his account, Marx's position in the *Ethnological Notebooks* represents an unfortunate break with this tradition. We have in this, his last work, evidence that he became an adherent of the *Gemeinschaft-Gesellschaft* myth, believing with Morgan that the ancient *gentes* (clans) provided the original model of the democratic and fraternal *Gemeinschaft* that constituted the polis (Krader, 1974, p. 3).[13]

Political society, as we have used that

term—i.e., polis based, urban, entrepreneurial, highly participatory, and in some cases even democratic—was typified by contractual relations of the most highly specified sort. It is significant, for instance, that the earliest Greek papyrus that we possess is a marriage contract of 311 B.C. from Assuan in Upper Egypt. We also have Aristotle's authority for the homology between social relations of the polis and partners to a business transaction. Even forms of friendship were conceived of contractually, concerned with the mutual exchange of goods and services, as Aristotle attests. The city-state in antiquity was itself more in the order of a voluntary association, and not coextensive with the inhabitants of a territory. One contracted in and out of the polis insofar as qualification for citizenship and formal admission by ratification of the phratry permitted; departure was marked by ostracism, voluntary exile, or death. As such, the *polis* constituted an aggregate of subordinate forms of association entered into by way of initiation rites, pacts, seals of blood covenant, etc., which enjoyed secret rites, private festivities, and legal immunity. As orientalists and classicists now concur, kinship or tribal organizations were, in the last analysis, voluntary forms of association rather than primordial networks. Recent work on tribalism in both classical antiquity and the modern Middle East suggests that tribe and clan are socially constructed entities that bear a tenuous relation to real blood lines, while trading off the moral resources of family and other forms of in-group loyalties.[14] Thus the designation of the tribe, named after a heroized ancestor, a mythical historical homeland, or an ancient totemistic sign, usually represents at the same time territorial claims, ethnic identification, and status, financial, or other aspirations on the part of the living association. In a relatively densely populated, commercially oriented city-state (as most Mediterranean and Middle Eastern societies have been from time immemorial) economic and social viability depend on a considerable volume of successful transactions of all kinds that is less significant in a decentralized agrarian society lacking the features of contiguous and mutually dependent existence. Oaths, pledges, promises, pacts, covenants, and contracts in a plethora of formulations and with all sorts of sanctions seem to have provided the guarantees on which such transactions depended, as witnessed by such documentary evidence as the Oxyrhynchus Papyri, oriental inscriptions, the Hammurabi code, etc. (Springborg, 1985).

The conclusion of this argument, then, is that *homo politicus* and *homo oeconomicus* represent not two phases in a process of social evolution, but rather alternative and contemporaneous ways of life with distinctive structures and institutions, differentiated regionally rather than chronologically. Moreover, these two regions have shown an amazing degree of continuity in their historical form. For instance, the sectarian warfare, institutionalized in the cultic, clan, and ethnic forms of association of modern Lebanon is reminiscent of the ancient skirmishing of Phoenicia, its historical parent state, of the agonistic struggles of ancient Greece, and of the civil wars of Rome, all of which Lebanon experienced historically more or less directly (Khuri, 1976). The ancient class, clan, and cultic forms of organization, the noble clubs, phratries, hetairies, and gymnasia, have their modern counterparts in the brotherhoods, professional syndicates, clans, and the religious and sporting clubs of modern Mediterranean society.[15] Where family and clan networks have proved to be ineffective they have been replaced by functional substitutes—for instance, reconstructed village associations that hark back to an original homeland, several generations and sometimes even countries removed; and associations formed by membership in the same graduating class of a high school, university, or military college (Gellner and Waterbury, 1977; Springborg, 1975, 1979, 1982; Khuri, 1975, 1980).

The renowned features of Mediterranean society as a shame culture hark back to a code of honour and protection of the in-group based on affinal networks against the out-group that have their counterpart in antiquity (Péristiany, ed., 1965, 1968, 1976). There too the *agon*, or contest for honour, was a constitutive feature. Performing the same function was an elaborate set of distinctions defining relations of social proximity, from immediate to extended family relations, friendship, and guest friendship, to the stranger, and backed up by private militias and blood feuds.

It has been said of ancient society that whereas modern life may be analyzed in terms of its public versus private dimensions, antiquity requires a tripartite division: public, private, and cultic. The elements which, when assembled, constituted the foundations of political society—family, clan, tribe, and village forms of subordinate association—were all religiously defined. This means that they were each associated with cultic rites and practices which created a differentiated set of common bonds for different purposes.

The substantive content of these cults was, curiously enough, provided largely from the Near East. There is a continuity of mystery and miracle cults, magic practices, forms of table worship, omen-taking, astrological lore, cultic incantations, and initiation rites that go back in some cases to Pharaonic and Babylonian practices of the third millennium B.C., and which were taken over and faithfully incorporated into their own religious practices by successive Semitic, Greek, and Roman invaders (Hooke, 1953). We have in the twentieth century seen investigations of some of the relics of these cults and practices, including the Coptic Gnostic gospels (Robinson, 1977; Pagels, 1979), the rituals of the Gnostic anti-Christian Mandaeans of the marshlands of modern Iraq (Rudolf, 1978), and the ancient secret rites of the Druzes, influenced as they were by neo-Pythagorean practices, as a direct link between ancient pagan oriental cults and their Hellenistic transmission (Makarem, 1974).

Because of the way in which the cultic dimension of social life influenced political and economic developments, this religious continuity in the Graeco-oriental world is of much more general significance. One of the distinctive features of ancient capitalism, as Weber observed, was the role of the notable in underwriting public works through liturgies (the funding of the army, the navy, and games and contests in ancient Greece), and the foundation of religious institutions (temples and mosques, funeral associations, and shrines for saints or martyrs, or *Waqfs*, as they are known in the Islamic world) (Veyne, 1976, pp. 232–79; Khuri, 1980, ch. 8). Because the urban, commercial, entrepreneurial society of the Mediterranean basin supported a wealthy class of notables more or less throughout its history who served local kings and emperors and foreign occupiers as municipal functionaries, and who underwrote public works at their own expense as the *quid pro quo* for social prestige, there was no need, and no room, for modern northern European post-Industrial Revolution capitalism to take root. The relatively small-scale enterprises of a relatively large class of notables shut it out. For this and other historical reasons, "modernization" and "development," where they have occurred, could only be undertaken by the state or the revolutionary party, because only they had the political and military muscle to override the indigenous commercial establishment. Even so, indigenous social forms have shown a characteristic ingenuity and durability in being able to honeycomb so-called "modern" imported institutions and structures with traditional affinal networks (Khuri, 1975; Moore, 1977; Springborg, 1975, 1979, 1982). It is the very durability of these ancient political forms, in the Middle East in general and in Lebanon in particular, that has presented to Western powers of a somewhat different historical experience some of their greatest challenges in the field of foreign policy.

Notes

I wish to thank Carole Pateman, Henry Mayer, Michael Jackson, Gian Poggi, John Burnheim, Gyorgy Markus and Maria Shevtsova, my colleagues, and Robert Springborg, my husband, for their comments on this paper and for discussions from which I have learned much. I owe a special debt of gratitude to John Pocock, whose comments ran to a small essay, Leszek Kolakowski, Preston King, Philip Rieff and Clement Henry, whose generosity in reading papers and offering scholarly advice on this and other occasions is greatly appreciated. Thanks are also due to Gabriel Almond, David Kopf, Philip Pettit, and Oliver Williams. My interest in Mesopotamian culture was rekindled during my time at the University of Pennsylvania, which financed many of the great excavations, including those of William Flinders Petrie, and which houses a magnificent collection of cuneiform texts, including copies of the Epic of Gilgamish.

1. The same is true, of course, for Soviet policy makers assisting third world countries that fall within their orbit to make the transition to socialism as the third phase in the Marxist version of the ubiquitous three-phase macrohistorical schema. Marx was as convinced an historical evolutionist as any of his nineteenth-century contemporary social scientists, and orthodox communism has promoted "modernization" by way of industrialization as ardently as the West.

2. Max Weber's account of the distinctiveness of modern industrial society, the locus of *homo oeconomicus*, compared with the society of *homo politicus*, the ancient polis, is still the most convincing and is remarkably unbiased by nineteenth-century romanticism which characterizes the distinctions of Morgan, Marx, and Tönnies. Observing that "whereas in Antiquity the hoplite army and its training, and thus military interests, increasingly came to constitute the pivot of all urban organization," Weber notes that "in the Middle Ages most burgher privileges began with the limitation of the burgher's military duties to garrison service." As a consequence, "the economic interests of the medieval townsmen lay in peaceful gain through commerce and the trades, and this was most pronouncedly so for the lower strata of the urban citizenry." Thus, he concludes, "The political situation of the medieval townsman determined his path which was that of a *homo oeconomicus*, whereas in Antiquity the polis preserved during its heyday its character as the technically most advanced military association: the ancient townsman was a *homo politicus*" (Weber, 1968, vol. 3, chap. 16, pp. 1353-4). Weber's theory, and indeed the bulk of his earlier writings, constitute an interesting commentary on Marx's epigrammatic observation that "the mode in which they gained a livelihood" explains why "for Athens and Rome . . . politics reigned supreme," but "not for the Middle Ages," in which "Catholicism played the chief part" (Marx, n.d., vol. 1, p. 86n).

3. Henry Sumner Maine (1861), dates the status to contract shift to the transition from tribal to agricultural society; Tönnies (1955, p. 274) to the transition from Graeco-Roman to Northern European cultural dominance; Marx, Weber, and modern sociologists to the transition from feudal to nation state in Europe (on the distinctions in Marx, Morgan, and Maine see Krader [1974, pp. 21, 36, 49ff.]). The classicist Sir Moses Finley (1955, p. 194), however, dates it to the transition from Homeric to classical polis society in Greece, seeing in the shifting marriage patterns of the Homeric world "merely one element in a much more general transition from an oikos-kinship world of status relations to the polis world of transactions consummated under the rule of law."

4. On the library of Ashurbanipal in Nineveh see Hooke (1953), Pritchard (1950), and Thompson (1928); on the Tell el Amarna letters, Petrie (1894), Mercer (1939), and Campbell (1964); on the Oxyrhynchus Papyri, see Grenfell and Hunt (1848-1967), and the two volumes of selected papyri in the Loeb Classical Library (Hunt and Edgar, 1932-34) which contain legal and other non-literary texts for the Hellenistic period; the first volume comprising private, the second, public documents. The Hammurabi Code—which is in fact less in the nature of a code than a set of amendments to third millennium Babylonian common law which the king promulgated in his own name—has now received definitive translation and legal commentary by Driver and Miles (1952-55), the latter of whom is a jurist. On the gnostic gospels see Robinson (1977) and Pagels (1979); on the Geniza documents see Goitein (1967-1983); on the Arabic texts of otherwise lost classical Greek and Hellenistic works of philosophy, science, medicine, etc., see Walzer (1962) and King (1978, 1980); on Sippar see Oppenheim (1969); finally, on the Ebla tablets, see Bermant and Weitzman (1979), and Matthiae (1980).

5. This remarkable chapter in the history of juris-

prudence is documented in the neglected study of Gierke, *Das Deutsche Genossenschaftsrecht* (Gierke, 1900, 1934). Arguing that "on the whole, the theorists of Natural Law were driven by the tendency of their time to deny that local communities and corporate bodies [i.e., the *collegia* and *universitates*] had a social existence of their own" as autonomous voluntary societies, Gierke concludes that "so far as it moved in this direction, the natural-law theory of associations was far less favourable to such bodies, on the fundamental issue at stake, than was the positive-law theory of Corporations" expounded by theorists of Roman civil law. Gierke saw in the social contract theories of Hobbes, Locke, and Bodin the logical outcome of this process by virtue of which "the sovereign State was directly based on the conclusion of a [fictitious] contract between individuals. . . . The Family and the State were therefore regarded as the only societies which possessed a basis in Natural Law," the family—*societas, privata,* or *domestica*—being subdivided into the "three societies of husband and wife, parents and children, and master and slaves," over which these theorists "hastened to set . . . at once the *societas politica* or *civilis,* a community armed with sovereign power. . . . [Thus] local communities and corporate bodies, as distinct from the Family, were regarded as only arising after the constitution of a system of political order" (Gierke, 1934, pp. 62–3). Gierke argues that this interpretation of the relation of subordinate forms of association to the state was based in part on a reading of the development of the classical *polis,* and probably Aristotle thereon, as a "progressive widening of the original family-community, first into the local community, then into the city, and finally into the greater kingdom . . . which made the local community merely a preliminary stage of the civic community, and treated the civic community as the perfect realisation of the idea of State" (Gierke, 1934, p. 63). A reading that it is my purpose in this paper to correct. On Hobbes and corporation theory see Springborg (1976). On the cultic *collegia* and *universitates* see Cumont (1956), Nock (1935, 1936, 1944), and Nilsson (1972). On the king as "corporation sole" see Maitland (1936, 1900), the latter comprising excellent essays on corporation theory and its ramifications in early modern thought, and also Kantorowicz (1957). On some peculiarities of social contract theory as it developed along these lines, see Gauthier (1977) and Pateman (1984a).

6. References to Marx's *Grundrisse* list first the page numbers of the German edition (1953) and second of the English translation (1973) unless otherwise noted. On the formative influence of the classics on Marx see Ste. Croix (1983, pp. 19–85) and Springborg (1984a, 1984b, 1984c).

7. Max Weber also considered the corporate nature of communal life to be the defining characteristic of the city as such. But he believed this community to be a feature of Western rather than Eastern cities. "Not every 'city' in the economic sense, nor every garrison whose inhabitants had a special status in the political-administrative sense, has in the past constituted a 'commune' (*Gemeinde*)," he maintained. "The city-commune in the full meaning of the word appeared as a mass phenomenon only in the Occident; the Near East (Syria, Phoenicia, and perhaps Mesopotamia) also knew it, but only as a temporary structure" (Weber, 1968, vol. 3, chap. 16, p. 1227). Weber goes on to argue, rather arbitrarily, that in the oriental city—in antiquity, and in particular under the great territorial kingdoms—"the associational character of the city and the concept of a *burgher* (as contrasted to the man from the countryside) never developed at all or existed only in rudiments." The "special status of the town dweller as a 'citizen,' in the ancient medieval sense, did not exist, and a corporate character of the city was unknown" (Weber, 1968, vol. 3, pp. 1227–8). He appears to make exceptions of those cities of Mesopotamia, Syria, and Phoenicia that grew up along the caravan route, which saw the "rising power of the patrician families in the 'city hall,' " for which he cites the Tell el Amarna tablets (Weber, 1968, vol. 3, p. 1230). But, because of its clan character, he completely misunderstood the character of Mecca, one of the most economically diverse and sophisticated oriental cities, observing that "all safely founded information about Asian and Oriental settlements which had the economic characteristics of 'cities' seems to indicate that normally only the clan associations, and sometimes also the occupational associations, were the vehicles of organized action (*Verbandshandeln*), but never the collective or urban citizens as such" (Weber, 1968, vol. 3, p. 1233).

8. Citations from classical authors, Plato, Aristotle and Herodotus, follow the standard form with page references to translations where they are not my own.

9. In this respect Islamic cities far exceeded the levels of economic development achieved by Greece and Rome. Goitein observes that the Cairo Geniza documents list "about 265 manual occupations . . . 90 types of persons engaged in banking and commerce and approximately the same number of professionals, officials, religious functionaries and educators" (Goitein, 1967–1983, vol. 1, p. 99). This total of around 450 professions for medieval Cairo, may be compared with the 150 professional corporations documented for ancient Rome, the 278 occupational corporations listed for Cairo in 1801, and the lexicon of some 435 recognized occupations compiled by the Qasimis, father and son, for Damascus beginning in 1890 and published only in 1960 (Goitein, 1967–1983, vol. 1, p. 99). It should not be forgotten that the Middle Eastern city has always been an urban conglomerate on a scale that the Greek *poleis* or the European medieval *metropoleis*

could hardly match, with extensive needs, therefore, for food and consumer durables. For instance, Babylon in the first millennium B.C. covered some 2,500 acres; Nineveh some 1,850; Uruk, 1,100; Calah, 800; while Athens at her height under Themistocles covered only 550 acres (Oppenheim, 1969, p. 5). The population of the Hellenistic cities, Alexandria and Constantinople, was in their prime around 200,000 souls, while that of Antioch is put at 100,000. For the Islamic period, the population of tenth century Baghdad is put at anywhere between 500,000 and 1,500,000, while Cairo in the fourteenth century was reported by a French traveller to be "twice as large as Paris [with] four times the population": now reckoned to have been 300,000 as against Paris's 90,000. In the modern period around 1800 the Syrian cities of Aleppo and Damascus had populations of around 200,000 and 100,000, respectively; Hama, Homs, Jerusalem, and Tripoli, 10,000 or more; while the total population of Syria was below 1,500,000. At the same time Baghdad was a city of around 100,000, the other Iraqi cities Mosul, Hilla, and Basra were all around 50,000; in a total population about the size of Syria's. Turkey in 1800, with a total population of over 10,000,000, boasted Istanbul at around 1,000,000, and Isfahan around 300,000. For a comparison the population of London in 1800 was 400,000 (Issawi, 1969, pp. 102–105).

10. Roussel (1976) and Bourriot (1976) have been hailed by classicists as having achieved a breakthrough in the study of ancient social structures, calling into question standard accounts of the transition from tribal to political society (see Gauthier, 1978; Fisher, 1979, 1981). English language scholars who anticipated the work of Bourriot and Roussel include Ferguson (1910, p. 283), who argued that the so-called clans (*gene*) were "ideal, not real, family groups" of the privileged, designed to look after the common interests of their members. In his specific study of the *genos* Salaminioi (1938, p. 42ff.), Ferguson showed this famous "family" to be a fictitious clan created to lay territorial claim to Salamis. Ferguson (1936, 1944) and Nock (1944), further emphasize the ceremonial and religious function of "familial" organizations.

Other significant works on family clan and tribe in antiquity are Wade-Gery (1931, pp. 131ff.), who argues that the phratry was an "ecclesiastical parish" based on religious cult and descent, the deme a "secular parish" based on locality and domicile; Costello (1938), who defends Aristotle on tribe, phratry, and *gene*, although seeing totemic origins for the clan; Pusey (1940); Andrewes (1961a, 1961b); Forrest (1960); Hammond (1961); and Davies (1977–78), on the phratry as a mechanism to ratify citizenship.

11. See Arthur (1973) and Saxonhouse (1980), both of whom conclude that women were better off under oikos-centered Homeric society than they were under the male polis. For a very interesting treatment of fraternal political relations see Pateman (1984b). Humphreys in particular observes:

> The subordinate segments of Athenian society, demes and phratries, though they borrowed all the forms of public procedure in the assembly, lacked its revenues and remained often under the sway of private resources and private interests. Private loyalties and, on occasion, conflicts were bound to be sharp in small patrilineally recruited groups. . . . Appeals to ties of kinship through marriage had no special status distinguishing them from appeals to ties of friendship. . . . For political alliance inside Athens it was not necessary to penetrate into the inmost private sector of the *oikos*, the women's quarters; the companionship of gymnasium and symposium were enough. . . . Within Athens, personal ties could be forged in the semi-private, semi-public masculine world of the symposium and gymnasium without recourse to marriage. The culture and traditions of the *hetaireiai* are more important than dynastic marriages. (1977–1978, pp. 98–101)

Costello (1938, p. 176) noted long ago that "the *hegaireia*, not the *genos*, was the characteristic organization of the aristocrats," whose fraternal relations were celebrated by Aristotle. Calhoun's work (1913), remains the best treatment of the *hetaireiai* and their manifold social functions.

12. An argument made explicitly by Roussel (1976, pp. 9ff.) with reference to Marx and Engels. Morgan, Maine, and Tönnies influenced a whole line of ancient historians and classicists, their legacy in the interpretation of the ancient *genos* having long outlived the credibility of the anthropological theories on which it is based. The elements of so-called tribal society which they isolate—divisions of the people into tribes and phratries, etc.—always in fact signified the constitution of a city (*polis*), as Max Weber himself observed (cited by Roussel, 1976, p. 5). Furthermore, the division of the citizens into tribes and phratries in Greece was almost always the work of a founder or reformer of the city.

13. That Marx underwent a change of heart between the *Grundrisse* and the *Ethnological Notebooks* is suggested by Hyndman, an English socialist with whom Marx had contact during 1880–1881, who noted in his autobiography: "Thus when Lewis Morgan proved to Marx's satisfaction that the gens and not the family was the social unit of the old tribal system and ancient society generally, Marx at once abandoned his previous opinions based upon Niebuhr and others and accepted Morgan's view" (quoted by Krader, 1974, p. 87, who makes nothing of this change of heart).

We have further evidence in the *Ethnological Notebooks* found in Marx's excerpts from Morgan on the Grecian *gens*, quoting George Grote (Marx, 1974, p. 201) to the effect that the *oikos*, extended family, was the foundation of Greek society, and that members of the same *gens* were not commonly related at all. For although the natural families which constituted the *gens* traced their origins to an eponymous common ancestor, this was a "fabulous geneology" and the *gens* was properly a cultic or religious form of association rather than a kinship unit. This view is supported by recent scholarship as well as by Niebuhr and Mommsen, Marx's recognized authorities in the *Grundrisse*, whom he now rejected on the authority of Morgan. In general, Marx finds little to disagree with in Morgan's account, making only one significant departure: he postulates antagonism within the *gens* due to the emergence of private property as a consequence of which "land, cattle and money capital had become of decisive importance with the development of slavery" (Marx, 1974, p. 213; Krader, 1974, p. 21). The contradiction between private and communal property, we know from the *Grundrisse*, was seen by Marx as the source of the decline of the ancient commune. Morgan also singled out the accumulation of private property and territory as the vehicle of transition from *societas* to *civitas*, but without postulating the internalization of conflict within the *gens*. Although Marx is considerably more skeptical of the work of Maine, he implicitly accepts the notion of the development of society as a transition from status to contract in the *Ethnological Notebooks*.

14. This is emphasized over and over again in Péristiany (1976). Emrys Peters (1976, p. 4) notes in his study of affinity in a Lebanese Maronite village that "affinity" is the "power to create groups"; Khuri, in his study of family associations in two suburbs of Beirut, notes the fictitious nature of eponymous clan organizations, "whose main function was to act internally, as a welfare and protection group and, externally, as a political entity, a receiver and dispenser of privileges":

> A family association is created by the founder registering the association and seeking to receive into it the largest possible number of those bearing the same family name while validating their claim to common descent by the fabrication of spurious geneologies asserting, in one instance, common descent from Abraham. (1976, p. 5)

15. On the role of wrestling clubs in Iran (descendants of the ancient *gymnasia*), see Bill (1973); on the Turkish *hammam* or baths, frequently a gift of the notables to the city as a continuation of the tradition of the *gymnasium* under the control of the *gymnasiarch*, see Veyne (1976, p. 365, n. 313). Several of the *Oxyrhynchus Papyri* (especially nos. 1416 and 1418) deal with the gymnasiarchy in Hellenistic Egypt (Veyne, 1976, p. 365, nn. 310, 314). The role of the Moslem Brotherhood, the Sufi orders, and the processional and funeral clubs as associational groups with dual religious and political functions in the Middle East (the latter in many respects the descendants of the famous Hellenistic funerary foundations), is too well known to require documentation.

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Forthcoming in June

The following articles and research notes have been scheduled tentatively for publication in the June 1986 issue:

Richard Born. "Strategic Politicians and Unresponsive Voters"

Steven Forde. "Thucydides on the Cause of Athenian Imperialism"

Milton Lodge and Ruth Hamill. "A Partisan Schema for Political Information Processing"

Rasma Karklins. "Soviet Elections Revisited: Voter Abstention in Noncompetitive Balloting"

Robert E. Lane. "Market Justice, Political Justice"

Arthur H. Miller, Martin P. Wattenberg, and Oksana Malanchuk. "Schematic Assessments of Presidential Candidates"

Edward N. Muller and Karl-Dieter Opp. "Rational Choice and Rebellious Collective Action"

Charles W. Ostrom and Brian L. Job. "The President and the Political Use of Force"

Arlene W. Saxonhouse. "From Tragedy to Hierarchy and Back Again: Women in Greek Political Thought"

Brian D. Silver, Barbara A. Anderson, and Paul R. Abramson. "Who Overreports Voting?"

Rein Taagepera. "Reformulating the Cube Law for Proportional Representation Elections"

S. Sidney Ulmer. "Are Social Background Models Time-Bound?"

Stephen K. White, "Foucault's Challenge to Critical Theory"

Gerald C. Wright, Jr. and Michael B. Berkman. "Candidates and Policy in United States Senate Elections"

CONTROVERSIES

THE EFFECTS OF LEADERSHIP SUCCESSION IN THE SOVIET UNION

What happens when there is a change in leadership in the Soviet Union? Does the period of leadership succession present unusual opportunities for domestic policy concerns to be vented? Or, are major issues on the Soviet domestic policy agenda addressed mainly after a succession of leadership is completed? Answers to such questions have a bearing on how the Soviet elite is replenished, and more generally help to improve understanding of Soviet politics. Two specialists on Soviet and East European politics, Valerie Bunce and Philip Roeder, have been drawn to different conclusions about these issues.

There is an ongoing debate among specialists in Soviet and East European politics about the impact of leadership succession on domestic policy priorities (see, for example, Breslauer, 1982a; Brown, 1982a; Bunce, 1980, 1981; Roeder, 1985). In my own work, I argue that the concerns of the public receive special attention during succession periods throughout the Soviet bloc (Bunce, 1980, 1981).[1] By contrast, Philip Roeder (1985) argues, for the Soviet case only, that such concerns rise on the elite agenda after succession has been resolved.

The purpose of this comment is to assess which argument best describes the Soviet case. This is not a trivial question. The precise relationship between "high politics" and "high economics" in the Soviet Union, as elsewhere, tells us a great deal not only about leadership succession, but also about the very nature of Soviet politics.

The logical place to begin the assessment is with the assumptions we make about Soviet elite politics within the Soviet Union. Roeder makes two dubious assumptions.[2] The first is that the first secretary always champions "mass" interests, whereas the rest of the Politburo always champions elite interests. This assumption, however, has little empirical support. Khrushchev's major rival, Malenkov, promoted light industry, a position resonant with mass concerns. Moreover, Brezhnev's policy stances over the course of his rule could hardly be said to support "mass" interests (see Breslauer, 1982b). All this would seem to suggest that it would be better to assume, as I did, that first secretaries vary in their policy orientations, and that decisions during and after succession are the outcome not of first secretaries "against all," but rather of more complex interactions among principal players driven by concerns with both power and policy.

Roeder's second problematic assumption is that the policy process during succession periods is essentially an elite "free-for-all." In my own view, new first secretaries, while assuredly constrained, are nonetheless *primes inter pares*. The evidence for my assumption is much stronger. If we read in between the lines of *Pravda*, we find clear indications that new first secretaries, while constrained, do dominate (see Breslauer, 1982a; and for the current succession, Brown, 1985; Gorbachev, 1985; Zagladin, 1985). Even more to the point, the power of new first secretaries is evident in their behavior—a far more concrete indicator of elite politics than veiled rhetoric. If intra-elite conflict is so pronounced, and if first secretaries are fighting for their political lives, as Roeder assumes, then why do new first secretaries always win?[3] Moreover, if conflict reigns and new first secretaries are impotent, then why do we see, during succession periods, so many innovations (which hardly speaks to a policy process immobilized by conflict), so many controversial decisions (which hardly speaks to the dominance of "elite" interests), and, finally, so many dramatic decisions that are clearly the work of the purportedly embattled first secretary? Just a few of the examples one can point to within the period from 1953 to 1957, the period Roeder identifies as the most conflictual of the post-Stalinist era, are the innovations Khrushchev introduced at the 1953–1954 Agricultural Plena, Soviet rapprochement with Tito, the substantial reforms in housing and welfare introduced from 1955 to 1957, and, of course, de-Stalinization. All this seems to indicate that a better assumption, and the one which guided my analysis, is that new first secretaries, while operating with a limited mandate and finite political and economic resources, have strong incentives and some opportunities for innovation during succession periods. To assume a "leaderless" process, therefore, is to ignore the concrete facts at our disposal.

The second area of disagreement between Roeder and myself is methodology.[4] The "improvements" Roeder offers have, in fact, the opposite effect. Let us turn first to his independent variable: the division of the period from 1953 to 1974 into six regimes (see his Table 3). The first component of this classification is shifts in the power of the first secretary. While I agree with Roeder that my dichotomy between succession and postsuccession periods is more "blunt" than the ever-changing world of high politics, I would counter that further temporal distinctions seem to uphold my original results (Breslauer, 1982a), and that such distinctions can only be examined through contextual, and not qualitative analysis (see Bunce, 1981, chs. 1–2, 6).

The second point is crucial. While most specialists can agree on when succession begins and ends in the Soviet Union, and this legitimates drawing a clear dichotomy between succession and postsuccession periods, the same can hardly be said for the additional gradations (by month!) Roeder provides. I do not share his belief that Khrushchev's power in April 1960 can be ranked numerically against his power one month later, or that Khrushchev's power in 1958 can be ranked against Brezhnev's in 1969. The data we have about the inner workings of the Politburo are necessarily subject to varying interpretations (compare, for instance, Breslauer, 1982a; Linden, 1966; and Rush, 1968).

Indeed, students of the presidency, privy to much more information, would shudder if asked to make similar "close calls" about the month-by-month changes in the power of Carter or, more generally, Carter's power in 1979 versus Ford's in 1975. Of course, Roeder evidences some discomfort as well. He overlaps years from one regime to the next, and the years he actually uses change both within the article in question (1960–1964 becomes 1962–1964) and from one article to the next (Roeder, 1984, 1985).

The problem of forcing rigor on bad data is also evident in his second measure of regime change: variations in levels of intra-elite conflict, as measured by elite survival rates. This is a poor measure. First, any small shift in (the controversial) timepoints would produce very different results. Second, it can be argued that survival rates may reflect governing style, not conflict—for example, Khrushchev's "distrust of cadres" versus Brezhnev's "trust in cadres" (see Breslauer, 1982a; Bunce, 1983), and may tap dynamics the opposite of those assumed by Roeder. For example, it can be argued that, rather than signifying the opposite, high turnover requires elite consensus.

Finally, even if we "pretend" that his regime boundaries and measures of power and conflict are all valid, we are still left with the serious problem that the results reported in Table 3—that is, the "final" regime rankings he employs in his analysis—do not add up. For example, according to his figures, column 9 ("mass interests") should register three ties, not the ranking from one to six he reports. Indeed, the most "mass-oriented" period in that column should not be 1957–1960, but rather 1953–1957 (the Khrushchev succession period, or what I originally argue!) and 1967–1970. Similar problems exist in column 10, where the most elite-oriented period, according to his numbers, should be 1953–1957, not the one he designates: 1960–1964.[5] And if we work with the correct results, we are left with the curious conclusion that one period, 1953–1957, is, despite a world of trade-offs, the most responsive to both elites and masses.

Roeder's methodological problems, moreover, do not stop with his independent variable; they are also evident in his measures of public policy and his assessment of policy change. At the bare minimum, an assessment of elite responsiveness to mass concerns requires (1) internally consistent data, (2) examination of a wide range of redistributive policies that can be traced to elite action, and (3) controls for temporal lags and trade-offs.[6] Roeder's analysis fails to meet any, let alone all of these preconditions, and this failure (not "ascendancy periods") explains his results.

The first problem is that Roeder relies heavily on budgetary and investment outlays, yet these data are not reliable for the timepoints he examines. If he interpolated the data, coded them as missing (which would still not reconcile differences over time or changes in budgetary scope), or subdivided them into smaller time frames, he should indicate that.

Second, Roeder confines his analysis to measures of policy that are easily quantified. This leads him, first, to mistakenly examine policies that are distributive, not redistributive—for example, growth in national income, sociocultural allocations in the budget (which includes distributive decisions on education, health, and science [see *Narkhoz*, 1976, p. 652]), and overall wage increases (instead of wage increases by class [see Bunce, 1980]). Moreover, it leads him to ignore programmatic changes. This is a serious deficiency. In contrast to shifts in funding and production, programmatic change has the decisive advantage of dealing with elite action and with redistributive issues. Moreover, such decisions occur on a precise date, and can serve as a check on patterns in less direct measures of decision making.

Indeed, if we examine changes over time in mass-oriented programs, it is clear that the "succession connection," and not Roeder's interpretation of postsuccession politics, is upheld. One can point, for example, to major expansions of the welfare state in 1955–1956; major increases in the minimum wage from 1953 to 1956, and in 1967–1968; significant boosts in rural living standards as a result of improvements in wages, pensions, social infrastructure, and state payments for

Table 1. Corrected Specification of Years: Annual Growth Rates, 1953-1961

Expenditure Category	Succession 1953-1958 (%)		Roeder Ascendancy 1957-1960 (%)		Correct Ascendancy 1959-1961 (%)	
Welfare Outlays (Total Budget)	11.2	(4.9)	3.1	(5.1)	5.8	(5.8)
Light Industry (Total Industry)[a]	10.2	(9.5)	15.5	(10.6)	1.1	(6.3)
Housing (Total Investment)[a]	21.8	(9.5)	8.1	(10.6)	1.4	(6.3)

Sources: Data sources for the budget are Pryor (1968, p. 375), and, for capital investment, *Narkhoz* (1959, pp. 548-51; 1962, p. 435).

Note: Roeder does not allow for lags or for changes in overall funding. When corrected, my conclusions are supported.

[a] Capital investment begins in 1956.

delivery of agricultural produce following the 1953-1954 and 1965 Agricultural Plenas; the Decree on Housing in 1957; and the impact of all these decisions on income distribution and the quality of life—improvements which seem to have stalled significantly after 1968 (see Bronson and Krueger, 1971; Bunce, 1980, Tables 1 and 2; Bunce, 1981, chs. 5-6; Colton, 1984, pp. 8, 70; DiMaio, 1974, p. 18; Hough, 1976; Maier, 1974; McAuley, 1978, pp. 344, 307-309, 316; Volkov, 1973). In fact, the "succession connection" is even evident in the Gorbachev succession. In May 1985, the Central Committee announced sharp increases in the number of children eligible for state aid, in the size of minimum pensions for *kolkhozniks* (about a 39% increase), and in state aid to the unemployable, single mothers, large families, and the "single" poor (see B Ts. K. KPSS, 1985).

The succession-related pattern in programmatic innovations is matched, moreover, by similar patterns in the quantifiable policies Roeder examines. He fails to pick these up and uncovers instead a postsuccession cycle because he fails to control for lags and trade-offs. More specifically, Roeder allows overall funding increases to distort his results, and sets up his "postsuccession" periods in a way that "captures" decisions made during succession periods.[7] Tables 1 and 2 below high-

Table 2. Annual Growth Rates, with Funding and Succession Periods Controlled

Expenditure Category	Succession (%)		Non-Succession (%)	
Light Industrial Investment (Total Capital Investment)	7.6	(5.0)	3.5	(4.3)
Welfare Outlays	7.9	(3.2)	3.8	(3.8)
(Total Budget)	7.0[a]	(2.4)[a]	5.6[a]	(9.5)[a]
Monthly Wages of Collective Farmers (Wages of Workers/Employees)	10.1	(5.2)	4.6	(3.2)
Housing Investment (Total Capital Investment)	7.7	(5.0)	2.7	(4.3)

Sources: The data are taken from Bunce (1980, Table 2); *Narkhoz* (1959, pp. 548-51; 1962, p. 435); and *U.N. Statistical Yearbooks* (1971, 1978, pp. 702, 881). Light industry was defined as the final six categories in industrial investment—that is, "glass through food preparation"—in *SEV* for 1965-75.

Note: The years are 1956-1977, though they sometimes deviate, given data problems. Succession periods are 1953-1958 and 1965-1968 (succession periods, proper, plus one-year lags for policy effects). Nonsuccession periods are the remainder.

[a] These data are from Pryor (1968, p. 375).

light the importance of these considerations. When proper controls and time points are used, succession periods again stand out as times when mass concerns receive special attention.

I must conclude, therefore, that my original argument stands. There is a "succession connection" in the Soviet Union and, for that matter, in Eastern Europe.[8]

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In the article under discussion (Roeder, 1985), I argue that the alternation found by Bunce (1980) between "mass-oriented" and "elite-oriented" policies in the Soviet Union can be explained by the conjoint effect of a general secretary's capacity to innovate and his incentive structure. These last two variables are, in turn, the consequence of shifts in the distribution of power and competition for power within the Soviet leadership. This argument revises Bunce's original "succession connection" thesis by finding a different temporal pattern in the alternation of elite- and mass-oriented policies, and by arguing that the general secretary's incentive alone is not enough to explain this pattern. Bunce has now responded with a pointed critique of that article. This exchange, by drawing out the points of disagreement between Bunce and myself, contributes to growth in our understanding of the Soviet system. Therefore, after a brief response to the major points in Bunce's critique, I will venture suggestions for further research arising from this debate.

Bunce's first criticism points up a central conceptual difference in our approaches: while she writes of general secretaries varying in their personal orientations, my own analysis stresses changes in the constraints on decision making. In the latter, one need not make the dubious assumption that all general secretaries undergo identical changes in personal orientation toward policies. Rather, recurring patterns of policy outputs and outcomes during successions are attributed to recurring patterns in succession politics that tend to induce general secretaries to make similar shifts in policy, even if their personal policy orientations remain constant.[9]

Closely related to this is Bunce's second point—our diverging conceptions of Soviet leadership politics. Bunce overdraws this difference. Although Russian history since at least the time of Svyatopolk the Accursed shows that successions in polities lacking strongly institutionalized procedures for the transfer of power can all too easily become "free-for-alls," the assumption of the inevitability of this is not part of my analysis. In fact, the analyses Bunce and I presented agree in key assumptions—specifically, that the general secretary has been the most important figure in the Soviet leadership throughout the period we examine, but that he does not rule alone; hence, the term "*first* among *equals*."

We disagree, however, in our diametrically opposite answers to the question whether one can ignore the impact of intra-leadership competition on policy outputs during successions. It is curious that Bunce should discount the importance of this, for the parallel between Soviet successions and democratic elections that she repeatedly draws suggests that competition among elites is central among the explanations for policy shifts. My own analysis rests on the claims (1) that intra-leadership conflict and the distribution of power within the leadership are variables; (2) that their variation is closely tied to successions; and (3) that changes in these affect both the general secretary's power to influence policy and his incentive structure. The evidence of elite contest within the Soviet leadership and its close association with successions is far more substantial than veiled disputes in *Pravda.* The removal and execution of Beria, the exile of Molotov to Outer Mongolia, and the ouster of Khrushchev, to name but a few, are strong evidence of such conflict. Contrary to Bunce's claims, general secretaries do not always win and front runners do not always come out on top, as the early removal of Khrushchev and the failure of Malenkov attest. The original "succession connection" thesis ignores changes in the level of elite conflict and in the structure of power within the Soviet leadership. It ignores changes in the power of the general secretary and examines only his incentive structure. Thus, the original "succession connection" thesis must implicitly posit either (1) that the level of intra-elite competition is constant or (2) that the changes in level of elite competition do not affect policy making and policy. Neither is a tenable assumption.

The third disagreement concerns methodology. Contrary to Bunce's attempt at caricature, my method does not fall prey to the many evils that she conjures. For example, it does rely to an important extent upon contextual analysis. The numbers confirm and reinforce this analysis; they do not stand by themselves. The rankings of regimes do not require close calls between individual years (or months!); the analysis is closely crafted so as to avoid exactly this.[10] Sovietologists have, indeed, made judgments about the power of the general secretary, and there is a surprising consensus in these judgments (e.g., Bialer, 1976; Breslauer, 1976, 1982a; Brown, 1978; Daniels, 1971; Hammer, 1974; Linden, 1966; Slusser, 1967).[11] Also contrary to Bunce's claims, Americanists have made similar—as well as more arcane—judgments about presidents (e.g., Barber, 1972; Chamberlain, 1946; Goldsmith, 1974; Spitzer, 1982). It is preposterous to fault the fairly conventional indicator of elite contest that I use (see Hough, 1976) by claiming that Politburo members in the process of removing or attempting to remove one another constitute a unified leadership. The alternative, of course, is the view that the Soviets themselves have attempted to present, yet few American political scientists would take this seriously. The numbers in Table 3 do not add up because the numbers are not supposed to be added—*the relationship is multiplicative!*[12]

Our disagreement on method results from Bunce's rejection of methodological rigor. Analysis of a causal relationship—here between successions and policy—requires that one use consistent data and control for exogenous factors that might yield spurious results. Bunce's use of proportions with changing denominators (e.g., budgetary shares) and her failure to control for demographic changes or secular trends make her results suspect. A slovenly methodology only compounds the problems of a deficient data set. A more rigorous methodology is, indeed, an improvement.

Last, it is curious that Bunce attacks the data I employ. In fact, most of the limitations of this data set are the consequence of an attempt to duplicate (for the purpose of a more rigorous retest) the indicators in Bunce's (1980) article. (I provide no explanation for recoding of data or subdivision of data "into smaller time frames" since neither of these procedures is used in my analysis.) Bunce's critique of my use of these data repeats the methodological failings of her original analysis, leaving one again to suspect that the results of her analysis are simply spurious. Moreover, in this critique, the dependent variable ("policy") becomes an even more imprecise concept than in the original statement of the "succession connection" thesis. It is now even less clear whether "policy" in this thesis refers to statements of intent, budgetary outlays, financial investments, or the outcomes of policy. And in this critique the relationship between succession and policy output becomes even more tenuous. For example, Bunce defends her original thesis by arguing that increases in spending occur only after the succession is over (that is, in the "period of ascendancy"), due to "lags." If "pumping up" consumer policies is a principal way in which a general secretary builds authority during a succession, in what way does spending two years later (after the succession is over) affect the outcome of a succession? If we accept Bunce's most recent revision of the original "succession connection" thesis, we end up with such silly arguments as that Khrushchev secured his tenuous position in 1956 by pumping up mass-oriented policies in 1958. (Of course, as my original analysis, but not Bunce's, makes clear, one must distinguish between the immediate response in policy outputs and delayed responses in policy outcomes.)

Central to the differences in our supporting data are our diverging standards of evidence. Both Bunce and I seek to identify short-term changes or shifts in policy priorities (mass vs. elite orientation) corresponding to succession and post-succession periods. Bunce seems content to search the pages of Soviet history and statistical annuals for illustrative evidence—e.g., isolated decrees—without any systematic attempt to compare the significance or impact of these with similar policies in other periods. My own analysis employs data that permit temporal comparisons with periods that precede and follow the ones in question. Only by such longitudinal comparisons can we observe policy changes. My analysis also employs data that provide precise definitions of policy outputs and outcomes, for only when we have defined a policy precisely can we observe change in it. Quantifiable data are, of course, not the only evidence that meet these standards; nor, as Bunce's use of statistics illustrates, do all quantitative analyses necessarily meet these standards. Yet with statistical sophistication, quantifiable data permit one to meet these standards in a parsimonious argument.

Rather than retreat behind defenses of our respective analyses, however, we might more profitably turn to the agenda of research suggested by this debate over Soviet successions and policy. Three items in particular have implications for the larger field of Sovietology: (1) Can we develop a more extensive set of measures of Soviet politics and policy that is consistent over time and permits more precise distinctions among policies (e.g., distributive vs. redistributive)?[13] Soviet budgetary expenditures and investments in particular are an important source of information about Soviet politics and priorities. Bunce's (1980, 1981) work is significant in that it begins the search for such indicators. My own work (Roeder, 1985) seeks to give greater precision to these.[14] (2) Can we expand nomothetic analysis within Sovietology? Contextual analysis, and particularly historical reconstruction, have been and probably will remain the mainstays of Sovietology. Yet Bunce's use of succession and nonsuccession periods as dichotomous variables that can explain policy priorities (independent of Zeitgeist or personality) is an important first step in the direction of theory building. I have sought to elaborate this theory. (3) Can we, through expanded comparative analysis, bring Sovietology closer to the theoretical developments in comparative politics? Bunce's work draws parallels between the "succession connection" of Leninist regimes and the "electoral connection" of polyarchies. My own work argues that parallels exist, but are more complex than those proposed in the original "succession connection" thesis. In short, this debate can be turned to a more constructive purpose.

The long-term outcome of this debate will be of interest to more than just Sovietologists. Our predictions of Soviet behavior—e.g., with regard to elite vs. mass orientation, immobilism vs. activism—have relevance for those who must formulate policies to deal with the Soviet regime. Our efforts to define more precisely what distinguishes a Leninist regime from a Western polyarchy (and in what ways they are alike) address concerns of comparativists, whether Sovietologists or not. Finally, our search for analytic rigor in both qualitative and quantitative analysis is a path already taken by many political scientists.

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Notes

1. I must note that Roeder's rendition of my work is incorrect in five regards. First, I do not characterize policy change during succession as either dramatic or as an iron law governing elite behavior (see Bunce, 1980, pp. 975-76; Bunce, 1981, chs. 5-7). Second, I do not equate campaigns in the East with those in the West—they differ in the composition and role of the electorate, the role of the issues, the sequencing of winning office versus mobilizing support, and, finally, in the nature of the mandate (Bunce, 1980; Bunce, 1981, chs. 1, 3-7). Third, I do not overlook constraints on the first secretary—I discuss at length the impact of conflict, policy specialization, and resource constraints (Bunce, 1980, 1981; and, for more recent assessments, Bunce, 1983). Fourth, I do not attribute my results to new leaders; instead, as a structuralist, I emphasize the interaction among (1) infrequent successions (which "locks in" interests and resistance to new ideas); (2) building ideological and political pressures to provide butter as well as factories, and (3) the role of the succession process in highlighting problems and legitimating some new policy alternatives. Finally, my point about mass unrest is that past experience is a powerful teacher—there was unrest, for example, in the Soviet Union, Czechoslovakia, and the GDR in 1953, and in Hungary and Poland in 1956. While elites differ on policy, they do not differ in their commitment to continued elite control or in their awareness that a prudent policy during succession periods is one which assumes unrest. To assume otherwise and be wrong would be far more costly than taking anticipatory measures by wooing the public.

2. Unless otherwise indicated, all references to Roeder concern his article in the December 1985 issue of this *Review*, "Do New Leaders Really Make a Difference? Rethinking the 'Succession Connection.' "

3. Brezhnev, for example, went through five American presidents.

4. I agree with Roeder that regression analysis would be preferable and that changes in the scope of the budget and plan present problems. However, Roeder hardly deals with the latter, since raw outlays are just affected as shares! Moreover, the "scope problem," along with problems in internal consistency, forces one to use abbreviated time-points. This, in turn, allows for distortions in regression outcomes—for instance, the exaggerated impact of one unusual year. For this reason, I left the data in a raw form which allows the reader, not the technique, to render a decision, and I examined programmatic change, which Roeder ignores.

5. Also, growth in heavy industrial investment (the primary indicator of elite interests) from 1953 to 1957 does not uphold his argument (see *Narkhoz*, 1959, pp. 548-51).

6. Roeder seems to assume that domestic policy is like foreign policy, in that changes in elite politics produce discrete decisions and carry immediate policy consequences (see Roeder, 1984). Such is not the case. The real world of budgets, for example, indicates that (1) decisions are made on the basis of total available funding and trade-offs among interests, and (2) there are long lags between changes in elite politics and shifts in funding, since budgets take a full year to be constructed and are enacted in December of the preceding year (Garbuzov, 1971; Yevdokimov, 1974). To assume that political changes in mid-1957, for instance, affect allocations in 1957 (retroactively?) or that increases in light industrial outlays can be isolated from increases in total industrial investment, therefore, is to assume away the real world of winners, losers, and time lags.

7. Roeder claims to examine elite interests. However, he examines elite interests for a three-year period only (1962-1964), fails to compare elite versus mass policies, and only looks at two "elite" policies: one rather removed from direct elite control (heavy industrial production), and one which is estimated by the West and exists separately from the overall decisional context (military outlays).

8. Roeder to the contrary, this includes agricultural decision making (see Bunce, 1981, ch. 6; Volkov, 1973). That policies of lesser significance have been introduced at times other than succession periods does not challenge the fact that the major

innovations in postwar Soviet agriculture occurred in 1953–1954 and 1965—that is, in succession periods.

9. In Bunce's analysis the key explanatory variable is a new general secretary, while in my own it is changes in decision-making processes that are affected by, among other things, successions. Failing to understand this conceptual difference, Bunce criticizes my assumptions about the policy preferences of general secretaries (for example, "the first secretary always champions 'mass' interests"), when in fact I make no such assumptions. The preferability of the latter approach is illustrated by the recent rapid succession of general secretaries from Andropov to Gorbachev. The logic of Bunce's analysis would have predicted a veritable explosion in mass-oriented policies as each new general secretary sought to secure his position. This, of course, did not happen. My own analysis would predict the very limited policy innovation throughout the protracted succession period beginning in 1982 that in fact has occurred.

10. The comparisons in my analysis are between periods of longer term. This "should not obscure the changes that take place within each period over time. In particular, toward the beginning and end of each period, decision making will exhibit many of the features of transition with a previous or new regime" (Roeder, 1984, p. 182). Of course, Sovietologists can, and frequently do, identify the month in which a succession begins or ends: for example, the death or removal of the former general secretary can be a good indicator of the former. In my analysis (Roeder, 1985), while the measure of elite competition is defined by month, the indicators of policy outputs and outcomes derived from Soviet statistical sources do not permit this; the latter are annual figures.

11. It is doubtful, however, that with the evidence available in the Soviet case one can separate, as Bunce attempts to do, "governing style" such as Brezhnev's "trust for cadres" from such constraints on that style as the extent of the general secretary's consolidation of power and the level of intraleadership conflict.

12. Bunce's point here rests on an unfortunate typographical error in the footnote to Table 3. Columns 9 and 10 are derived by multiplying Column 6, not 5, by Columns 7 and 8, respectively. All columns show the rank orders of the products.

13. It is curious that Bunce's critique should emphasize the distinction between distributive and redistributive policies, for I introduce this distinction as an anomaly in Bunce's indicators that she completely ignores.

14. Many of Bunce's criticisms of my indicators—which are also her indicators—imply that "better" and therefore different data are needed. While this would be inappropriate in an initial retest of the original "succession connection" thesis, it is undeniably a long-term need in the study of Soviet policy.

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EXPLAINING AFRICAN COUPS D'ETAT

Comparative political analysts seek empirical generalizations which will hold water across systems and over a period of time. Yet, modeling important political phenomena over more than a handful of countries is still rather unusual. One focus for substantial comparative research has been the coup d'état—an irregular change of governmental leadership by force—in African countries. Scholars who have engaged in this research find they have various conceptual and methodological differences of opinion. In this Controversy, *Robert Jackman and Rosemary O'Kane raise the issues in dispute. Their contentions are answered by Thomas Johnson, Pat McGowan, and Robert Slater. The exchange highlights important research issues without necessarily resolving them.*

In a recent paper, "Explaining African Military Coups d'État 1960–82," Johnson, Slater, and McGowan (1984) replicate and attempt to extend my earlier work on coups in sub-Saharan Africa (Jackman, 1978; see also Jackman and Boyd, 1979). They present an alternative model that seeks to account for the total volume of coups that occurred in Africa in the years from 1960 to 1982. Evidently, Johnson et al. are well satisfied with their extended model. It forms the basis of attempts (a) to examine African coups from a more "historical" perspective (McGowan and Johnson, 1984); (b) to forecast more recent coups (McGowan and Johnson, 1985a); and (c) to simulate the incidence of coups (Johnson and McGowan, 1985). Their paper in this *Review* makes two general points. First, Johnson et al. imply that I was arbitrarily selective in defining the appropriate set of countries for my analysis, and they report that when "six new African states" (1984, pp. 630–31) are added to the analysis, my model breaks down completely. Secondly, Johnson et al. offer a new model they claim accounts "in a theoretically meaningful fashion for 91% of the variation in military coups" (1984, p. 621).

My purpose is to show that Johnson et al. have made a series of fundamental mistakes.* Their discussion of case selection is at odds with the basic canons of comparative analysis. Their new model suffers from crippling problems of measurement validity and of specification error. As a result, their analysis contributes nothing to our understanding of the forces that underlie coups d'état.

Selection of Cases

The analysis in Jackman (1978) sought to account for the total volume of coups experienced by the new states of sub-Saharan Africa in the years from 1960 to 1975. Given the substantive concerns and the period involved, what set of states does this include? I restricted my attention to members of the Organization of

African Unity located in sub-Saharan continental Africa. This is consistent with standard treatments (see e.g., Collier, 1982; Morrison, Mitchell, Paden, and Stevenson, 1972), and indeed, Johnson et al. also exclude the North African countries of Algeria, Egypt, Libya, Morocco, and Tunisia on the grounds that these countries belong to "the Middle Eastern, Arab-Islamic culture complex" (1984, p. 636). However, unlike Johnson et al., I also excluded Madagascar, on the grounds that its political history is distinct from that of the continental black African countries in much the same way as the political history of North Africa is distinct from that of sub-Saharan Africa. This doubtless accounts for the exclusion of Madagascar in analyses by other researchers as well. In addition, Ethiopia was excluded from the main part of the analysis because of its lack of political parties circa 1960. Johnson et al. "find this reasoning acceptable" (1984, n. 5), but then unaccountably include Ethiopia in their own analyses. They report that the inclusion of Ethiopia had no major effect on the estimates of my model (see the third column of their Table 5), though the lack of political parties in Ethiopia, circa 1960, makes one wonder how they generated a party dominance score for this country. Inclusion of Ethiopia can hardly be justified in the context of "replicating" my model.

This leaves four countries excluded by me but included by Johnson et al.: Botswana, Lesotho, Mauritius, and Swaziland. According to Johnson et al., I excluded the first two of these "without offering any justification" (1984, p. 629). But, the original paper referred to the data collected "for the 30 countries listed in the Appendix (*all of these countries were independent by 1965*)" (Jackman, 1978, p. 1264, emphasis added). All four countries listed above became independent after 1965, and were therefore excluded. Why was 1965 selected as a cutoff date?

In answering this question, it is useful to remember that a coup d'état is, literally, a strike against the state. This implies that a coup can only take place within a state. It should be evident that colonies are not states—hence, for example, the literature in the early 1960s on the rise of the "new states" (see, e.g., Emerson, 1962; Geertz, 1963). Colonies cannot experience a coup d'état.

This last point is important, because before we can make comparisons across units (in this case, nation states), we have to be sure that each unit is potentially capable of exhibiting the attribute of concern (see, e.g., DeFelice, 1980; Jackman, 1985). Despite statements to the contrary (e.g., Sartori, 1970), the units need not have actually exhibited the attribute (in this case, coups) to be compared, but they do have to be potentially capable of it. This criterion for the inclusion of cases directly parallels the demographic and actuarial notion of a "population at risk" (see, e.g., Barclay, 1958). Suppose we wish to calculate an infant mortality rate, and that we have, for a given period and unit, the number of infants born who died before age one. To calculate the infant mortality rate, the relevant denominator is the total number of births in the same period and unit (the population at risk). When examining coups d'état, colonies are not part of the population at risk.

In light of this, and given my concern with the years from 1960 to 1975, the focus clearly needed to center on states in sub-Saharan Africa. At the same time, it is desirable for a variety of reasons to maximize the sample size. In the tradeoff between maximizing sample size and restricting the focus of analysis to states, my compromise was to exclude states that were not independent by 1965, so that all cases would be potentially capable of experiencing a coup for at least two-thirds of the period under consideration. Thus, the data covered the various forms of coups for the years "from independence or 1960 (whichever came later) through 1975, inclusive" (Jackman, 1978, p. 1264). Any compromise may be subject to criticism. But the most reasonable criticism of this decision would be that it leaned too much in the direction of maximizing sample size, so that 1965 was too late a cutoff.

The criticism offered by Johnson et al., however, comes from the other side of the field—that 1970 (or 1968 in practice) is the defensible cutoff. (Indeed, they seem to imply that my definition of the set of appropriate countries was intentionally selective.) They further report that when the 1970 cutoff is employed, and Ethiopia and Madagascar are also included, my model suffers a "nearly complete breakdown" (1984, p. 629). I agree that under these circumstances my model fails from a statistical perspective, but what meaning should we attach to this fact?

The only reasonable inference is that my model does not apply to those cases to which it was never meant to apply—namely, Ethiopia, Madagascar, and four other states that became independent after 1965 (two in late 1966, and the other two in 1968).[1] I have already discussed the rationale for excluding Ethiopia and Madagascar. The remaining cases were not considered because they were not potentially capable of experiencing a coup for up to one-half of the period under consideration. Since they do not belong in the analysis, the failure of the model brought on by their inclusion is an inappropriate test of validity of that model.

The New Extended Model

Throughout their paper, Johnson et al. refer to their new model as a "theoretically informed" model. Presumably, this means that general arguments and concrete justifications are offered for the specific empirical model that is estimated. What is the broad substantive rationale for the model? What are the critical variables? What is the appropriate structure of the model to be estimated? How, and for what dates, are the critical variables measured? Do the particular measures chosen offer a reasonable representation of the variables of theoretical interest? Johnson et al. devote rather little attention to the specification of their empirical model. Instead, they report examining "a number of alternative sub-models," the details of which are not supplied. Then, they report: "Combining the results of our sub-model analyses permitted us to specify and to estimate the regressions reported in Table 7" (1984, pp. 633–34). This paragraph is the only discussion of how the empirical model was derived. What implicit theory informed the model? Certainly there is no explicit theory.

According to the model, the total volume of coups (1960–82) is said to be a function of (a) characteristics of the military, (b) political development or lack thereof, (c) social mobilization, (d) national political economy, and (e) international economic dependence. At a very general level, this appears to be reasonable. However, when one examines the exact measures used and the variety of dates to which they refer, the basis for the specified model is not at all clear. Indeed, the specific measures chosen are nowhere justified. They are simply listed (all 10 of them) in footnote 12 on page 634.

Additionally, while the dependent variable covers the period from 1960 to 1982, the "explanatory" variables refer to a variety of dates: e.g., 1950–60, 1975, and 1960–78. No explanation of this variety is offered. Even constraints of data availability cannot account for all of this temporal variation. As I show below, this introduces major potential problems of simultaneity bias.

The specification offered by Johnson et al. is indefensible, and most of the explanatory variables do not belong as part of an "explanation" of coups in the period. The only way to demonstrate this is to consider each of the explanatory variables in turn, so as to get a sense of what this model is trying to tell us. This approach may seem a little data-centric, but the model appears to have been developed in a very data-centric manner. The lesson to be learned is that the statistical superstructure proposed by Johnson et al. rests on a weak conceptual foundation.

Characteristics of the Military Itself

Johnson et al. represent the military in terms of *army cohesion* and *army political centrality* (1984, Table 7). *Army cohesion* refers to "militaries that are large *and* ethnically homogeneous" (1984, p. 634, emphasis added). How is it measured? By taking the product of the size of the largest ethnic group (as a percentage of the population in the country as a whole) and the size of the military (total military manpower in thousands). Does a significant coefficient for this measure tell us anything about whether militaries are large and ethnically homogeneous? No, it does not. A high score could reflect this for a given country, but it could just as easily reflect a case where a smaller ethnic group is politically dominant and constitutes most of the army. The second outcome is not uncommon in African politics (remember Idi Amin?). What we have here is one version of the "ecological fallacy" (to use the old term). Johnson et al. to the contrary, their measure simply reflects (1) the raw size of the military, and (2) the ethnic diversity of the population as a whole. It tells us nothing about the ethnic composition of the military.

Because it is the product of military size and ethnic diversity, army cohesion actually represents an interaction between these two variables, with the "main" (additive) effects of military size and ethnic diversity assumed to be zero. In other words, Johnson et al. are proposing that neither of these two components is important in itself, but countries where the military is large and the largest ethnic group is numerically dominant are especially prone to coups. The rationale for any such expectation is neither provided nor self-evident.[2]

One other point: Army cohesion is measured for circa 1970. There is a nontrivial problem in using this to "explain" coups starting in 1960, given that one can plausibly argue that after coups, the size of the military increases. The treatment of this variable by Johnson et al. runs a considerable risk of confusing cause with effect. As I show below, this is also an issue with some of their other "explanatory" variables. Minimally, one would have expected them to entertain the possibility of a reciprocal relation between coups and army cohesion. As it stands, their use of this variable (measured as it is for around 1970) in a recursive format raises a problem of simultaneity bias.

Army political centrality is said to reflect the degree to which the military is "relatively central because of its role in repression and because of its claim on state revenue" (1984, p. 634). The actual measure is again a product, this time of (1) government sanctions (1960–67) and (2) defense expenditures as a fraction of total government revenue (1965).[3] Those who have worked with the sanctions data (Taylor and Hudson, 1972) know that they are a potpourri that include military actions as well as the activities of other political actors.

The broader point again concerns probability simultaneity bias. On the average, military governments are probably more repressive than non-military ones. Indeed, when one codes military coups from primary sources, one tends to notice that they are typically followed by martial law, arrests, banning of political parties, etc. There is systematic evidence to support the alternative specification that I am suggesting (e.g., Hibbs, 1973).

A similar argument can be made for the size of military budgets. Including both components in a multiplicative term makes the resulting measure difficult to interpret, and certainly does little to alleviate the wider potential problem of simultaneity. Labelling the resultant measure "army political centrality" is simply misleading.

Political Development or Lack Thereof

The claim that their model examines the effects of political development is questionable in view of the fact that development is never actually measured and incorporated into the model. Instead, we are offered measures of turnout (*turnout*) and of degree of multipartyism (*political pluralism*). I recognize that there is a good deal of debate over how development is best measured, but a reading of Huntington (1968) and the subsequent literature suggests these may not be optimal measures.

Turnout refers to pre-independence electoral turnout, and was also employed by Jackman (1978). It identifies the amount of participation in the last election supervised by the relevant metropole. One can readily justify the inclusion of this variable, but the construction that Johnson et al. put on it is dubious: "African states . . . which have *maintained or restored* some degree of political participation. . ." (1984, p. 635, my emphasis). The measure that is actually employed has nothing to do with the restoration of political participation after decolonization. Instead, it identifies part of the colonial legacy.

Political Pluralism: The inclusion of this variable, based on a measure of party fractionalization, is extraordinarily hard to justify, since it identifies fractionalization as of 1975.[4] Note that the t-ratio for the estimate of political pluralism in Johnson et al. (1984) is about 8.5, which is larger than any other t-ratio reported for the model (see Table 7). But the estimate is impossible to interpret. How can one logically claim that party fractionalization as of 1975 caused coups starting 15 years earlier?

Remember that in the African case, party fractionalization since independence has exhibited considerable short-term volatility, a good deal of which is caused by coups. After the typical coup, one of the first orders of business is to ban all political parties, or at least everyone else's. This tends to generate a negative correlation between the number of parties (1975) and the number of coups (1960–82), since 1975 is closer to the end of the period 1960–82 than to its beginning. This particular causal inference by Johnson et al. is ludicrous.[5]

Social Mobilization

Johnson et al. (1984) represent social mobilization as *percentage in agriculture* and *percentage in the capital city*. *Percentage in agriculture* is said to refer to the percentage of the population in agriculture (1960). Actually, it refers to the percentage of the labor force employed in the agricultural sector, and there is a nontrivial difference between the two denominators. In any event, Johnson et al. appear to have used the correct variable, but mislabelled it.

Percentage in the capital city refers to the percentage increase in the population of the capital city (1950–60). There are two problems here. While Deutsch (1961) and others do include urbanization as a component of mobilization, they do not restrict their attention to capital cities. There is no good reason for such a restriction, even though coups take place in capital cities for obvious reasons. Specifically, consider what social mobilization is supposed to involve—the erosion of older, more "traditional" attachments and the substitution (if not necessarily full acceptance) of newer attachments among the public. In fact, urbanization is often a dislocating experience. But what is so distinctive about capital cities? The measure used here implies that urbanization only "counts" when the capital city is involved, but why should this be the case? For example, does the meaning people attach to the under- or unemployment that often accompanies mobilization vary according to whether it is experienced in the capital as opposed to some other city? Secondly, why is change from 1950 to 1960 taken as the relevant quantity to influence coups from 1960 to 1982? No justification is offered, and none is readily apparent.[6]

National Political Economy

Growth in GNP, growth in industrial jobs, and *exports-imports/GNP* are the variables used by Johnson et al. (1984) to represent the national political economy. *Growth in industrial jobs* refers to growth in the percentage of industrial jobs from 1960 to 1978, and is taken from World Bank sources. *Growth in GNP* refers to growth in GNP from 1965 to 1970. There are two problems here.

First, if 1960–78 is the relevant period for growth in industrial jobs, why is it not also the relevant period for growth in GNP? The source for the growth in GNP measure provides data covering the years up to 1975. Why were these data not used? Even better (for the reasons set forth in Jackman, 1980), why not use the available World Bank estimates based on continuous series for growth that would take us up to 1978 or 1980?

Exports-imports/GNP refers to the ratio of exports–imports to GNP (1965). Although no rationale is offered for this particular measure of economic performance, one can at least think of one for it. However, why is 1965 the relevant date, given the periods to which the other two measures of economic performance refer? The present treatment of the dates is inelegant, to say the least, especially considering that the dependent variable covers the period from 1960 to 1982.

Secondly, and much more importantly, there is again a potential simultaneity bias here. While military regimes may not have a distinctive impact on economic growth, there is good reason to expect political instability to have a deleterious effect on growth, and the "dependent" variable in this analysis measures political instability, not degree of military influence in politics.

International Economic Dependence

Change in commodity concentration is the term used by Johnson et al. (1984) to refer to change in export commodity concentration from 1960 to 1965. This is a reasonable measure of one aspect of dependence, but it is not clear why 1960–65 is the relevant period. Moreover, it is not clear why this is the relevant dimension of dependence for the problem at hand—why, for example, is this measure more germane than the degree of economic penetration by multi-national corporations?

A final note on the measures. At the end of footnote 12 (Johnson et al., 1984, p. 634) we are told that "in a few [unspecified] cases the *mean value of the variable was used to estimate otherwise missing data*" (my emphasis). While this procedure can sometimes be justified, it strikes me as quite unwarranted in the present context, especially given that the total N is 35.[7]

To summarize, the model proposed by Johnson et al. is misspecified. I would like to underscore the point that substantive justification is needed for the inclusion of each measured variable in the model. Unfortunately, such justification is lacking in the analysis by Johnson et al. Several of the critical measures are invalid representations of the concepts they are said to reflect, and the inclusion of only one bad variable can distort the remaining coefficient estimates in the model. The dates to which many of these measures refer are highly variable, for no apparent reason. Coups (1960–82) are "explained" by variables referring to circa 1965, circa

1960, 1975, 1960, 1950–60, 1960–78, 1965–70, 1965, and 1960–65. Finally, there is a problem of major proportions having to do with simultaneity bias that affects many of the critical variables. This means that no clear inferences can be drawn from the model as specified.

What Is To Be Done?

Where does this leave the analysis of coups? Let us start by returning to my original model, the purpose of which was to estimate the effects of initial social and political conditions on the subsequent volume of coups for countries in sub-Saharan Africa.

Why is this an interesting question? The new states of sub-Saharan Africa share a number of characteristics deriving primarily from the colonial experience and the process of decolonization around 1960. Because these countries experienced decolonization at approximately the same time, they form an important setting in which to evaluate arguments about political development. First, these countries form part of the periphery of the so-called world economy (cf. Kick, 1984; Snyder and Kick, 1979), and a good deal of attention has focused in recent years on the prospects for development and growth in the periphery. Secondly, that they are new states means that they are relatively weak on at least one aspect of institutional capacity (cf. Huntington, 1968). Thirdly, the colonial experience was by no means uniform, as becomes clear when one contrasts, say, Kenya with Zaire.

Examining the effects of initial social and political conditions on subsequent levels of political instability is important, in part, because those initial conditions reflect aspects of the colonial legacy itself. While the metropoles did not have total control over the last election held before independence, their activities certainly had considerable bearing on those elections, as reflected by the level of participation and the forms of the party systems in those elections. Similarly, the nineteenth-century "scramble" for Africa has obvious implications for contemporary variations in degree of cultural pluralism across the African states. The variety of colonial experiences also has a clear bearing on the variation across these states in the levels of social mobilization they exhibited around the time of independence. My concern was to evaluate the significance of these differences for subsequent levels of instability.

The estimates reported in Jackman (1978) indicate that these initial structural conditions had a pronounced effect on instability from circa 1960 to 1975, which is a relatively long period given the potential temporal volatility of events like coups d'état.[8] The estimates reported by Johnson et al. (1984, Table 6, col. 3) indicate that most of these effects persisted when the period was extended through 1982, albeit at a weaker level.[9] I did not anticipate that the effects would persist forever. That they held up through 1975 is noteworthy in itself, but, like all empirical political research, my analysis is clearly time-bound. However, there is nothing in the Johnson et al. analysis that modifies my original conclusions about the structural sources of political instability in sub-Saharan Africa from 1960 to 1975. Nor does their effort advance our understanding of coups in more recent years.

I have emphasized my concern with the effects of initial conditions on the volume of subsequent events. This concern was underscored at several points in the original paper. Before one can draw reasonable causal inferences, problems of temporal sequencing need to be addressed. If they are not, the potential for simultaneity bias that plagues the model offered by Johnson et al. is unavoidable.

Moreover, my purpose was to examine the structural forces that generated a volume of instability in the set of countries and period at hand. Johnson et al. opine that since coups are inherently "dynamic" events (1984, p. 637), we ultimately need to engage in time-series analyses of these events if we are to develop good models. This is misleading, because dynamic properties inhere not in events or attributes, but in how we study them.

It is important to remember the distinction between the long-term structural forces and the short-run precipitants that underlie coups d'état. The former may help to determine which countries are more prone to coups, but obviously do not address the issue of when a coup is likely to occur in a given country. While cross-sectional analysis is a useful mode of analysis for isolating relevant structural forces (especially those like cultural pluralism that exhibit considerable temporal stability), it is less useful in understanding the precipitants of coups (cf. Jackman, 1985). Examining the latter issue requires both more of a temporal focus and (perhaps) a different set of explanatory variables. The two forms of analysis address distinct questions, the answers to which should complement each other.[10]

Finally, Johnson et al. complain that my model is "rather mainstream" (1984, p. 632), in the sense that it ignores characteristics of the military and economic dependency. On the first count, I believe that earlier analyses have overrated the significance of military organization for coups d'état—certainly, Johnson et al. provide no firm evidence to the contrary. Instead, I think that coups are fundamentally political in nature. And if one contemplates events like the coup of 1963 in Togo, it is not at all clear why organizational characteristics of the military should loom so large in explanations of coups. On the second count, it needs repeating that a focus on sub-Saharan African states immediately centers one's attention on the periphery of the world economy. To attempt an explanation of coups in the African states involves ipso facto a consideration of the prospects for political development in the periphery. If this is mainstream thinking, the field of comparative politics has a brighter future than I had realized.

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In principle, Johnson, Slater, and McGowan's (1984) "Explaining African Military Coups d'État, 1960–1982" advances our understanding of important political events in true Popperian manner. It takes a well-respected hypothesis about African coups (Jackman, 1978), replicates it, adjusts it, and improves on it, taking the condition of our knowledge and ability to explain coups d'état a significant step forward. In practice, unfortunately, it does none of these things, and seems likely to succeed only in diminishing the perceived value of statistical analysis for the already skeptical African specialists. In their article, Johnson et al. take a hypothesis about coups in tropical black Africa, apply that hypothesis only to military coups and to countries that are not all tropical black African countries, and produce results different from Jackman's. Claims are then made for the importance of these results beyond their limitations. In so doing Johnson et al. demonstrate a lack of appreciation of the part played in coups by the military; an ignorance of some African and some European countries; a failure to appreciate the implications of the assumption of ceteris paribus; and last but not least, an insensitivity to the dangers of tautology. These considerations point clearly to the value of statistical analyses of models generated by theories as opposed to a collection of "best fit" hypotheses. Furthermore, they reawaken Zolberg's (1968) approach, which both Jackman and Johnson et al. have sought to undermine. On balance they also support rather than disprove Jackman.

To begin, then: whether all coups or only military coups should be studied will depend on the nature of the hypothesis to be tested: Jackman's model was concerned with coups, not the narrower category of military coups. Separating military from nonmilitary coups is a highly dubious exercise. An accepted part of the definition of a coup is that it is a strategy for government overthrow which involves the threat or use of violence.[11] That the military should play such an important part in coups is not surprising. The definition of a coup crucially suggests that no particular significance should be attached to the military playing an active part in it. Where violence needs only to be threatened rather than used, the military may appear not to have been involved when its support has in fact been critical.[12] However, if it is the military's involvement in Africa, rather than coups, which seems interesting, then a different problem arises. Military personnel can be given government posts, and often are placed over affairs of defense especially. The threat or even supposed threat of intervention could in any case be enough to ensure that a civilian government ruled in the interests of the military. It should not be forgotten, too, that in Africa the emphasis on the importance of the military in threatening violence may be entirely misplaced, for not all African countries have had a standing army for the entire period of their independence, and some still have not (*Europa Year Book*, 1982). The police, for no other more significant reasons, may therefore be the important force in these African countries.

That the causes of African coups differ from the causes of coups in other areas of the world is a reasonable proposition; it is also a testable one. African countries generally share a colonial heritage, recent independence, and tribalism, and suffer from conditions of economic backwardness (low GNP, export dependency on primary goods, and the like). There are also important differences. Johnson et al. emphasize the difference between the northern Arab states and the black African countries on which they concentrate (that is, those independent before 1970 and not under minority rule, such as is the case in Namibia and South Africa). It is a theme which seems to follow Jackman. Johnson et al., unlike Jackman, do not seem to appreciate the theoretical support for the separation out of a particular set of countries. Zolberg (1968, p. 71) makes a strong case for the existence of important shared conditions amongst the "newly independent countries of Tropical Africa": "extremely weak national centers, a periphery which consists of societies until recently self-contained, and levels of economic and social development approaching the lowest limits of international statistical distributions." In using data from only tropical African countries, Jackman performs an experiment to discover the difference between coup and noncoup countries under the assumption of ceteris paribus (other things being equal). He therefore correctly removes Ethiopia from his final predictive model for not being "newly independent." Ethiopia, unlike all the other tropical African countries, was never colonized, only being occupied by Italian troops for the short period from 1935 to 1941. Johnson et al. do not accept the importance of this ceteris paribus assumption, and are shocked by Jackman's failure to include Madagascar, Mauritius, Swaziland, Botswana, Lesotho, and Ethiopia. The reason these countries were excluded is, of course, because none of them share Zolberg's similar backgrounds in being tropical African states. Two of them are not even black African states.

Ethiopia's lack of a colonial history has already been mentioned. The southern

African countries (Botswana, Lesotho, and Swaziland) differ in crucial respects. Their economies, both import and export, are heaviy dependent on South Africa; they are also dependent on South Africa for external defense. They have weak central governments in the sense of being dominated by South Africa, but not "weak national centres" in Zolberg's sense. They are not intensely split by tribes. Mauritius is not even a black African country, with two-thirds of the population being of Indian descent and the remainder being either European (mainly French) or a mixture of Indian and European. It is also hard to view Madagascar as a black African state when the majority of its population is of Malaya-Polynesian stock and its homogeneous Indonesian/African culture is unique (Kurian, 1982). Both, of course, are in the Indian Ocean and not on the mainland. The only thing which Mauritius seems to have in common with the other new countries in Johnson et al.'s data is that since independence it, much like Lesotho, Swaziland, and Botswana, has had an armed police force rather than an army. Mauritius still has no standing defense force, only a special police mobile unit. In Lesotho a plan for converting the paramilitary force into an army was only announced in 1979. Botswana did not establish an army until 1977, while in Swaziland this was done in 1973. Johnson et al. (1984, n. 10) note: "Four of these states—Botswana, Lesotho, Mauritius and Swaziland—are among the most stable in Africa and have never recorded a single military intervention event." While Botswana has an impressive record of stability, this is not so true of the other three countries. Mauritius has been described as being "plagued by political, industrial and racial unrest" (*Europa Year Book*, 1982, vol. 2, p. 928). In both Swaziland and Lesotho, in 1973 and 1970 respectively, the constitution was suspended and opposition leaders were imprisoned. Those events in Lesotho are often described as a coup; in Swaziland it took until 1978 before proper elections were held. The failure of attempted coups were reported in Lesotho in 1974 and Swaziland in 1984, and coup plots were alleged in both countries at the end of 1983.

Whereas Jackman has performed a ceteris paribus exercise, Johnson et al. have performed a test on a collection of countries that neither share the same backgrounds nor are randomly sampled. Indeed, they are a very odd bunch of extra countries. While not sharing Johnson et al.'s view (1984, p. 637) that the poor quality of data for countries with populations of under one million would normally justify their being excluded from cross-national research (small countries with open societies can collect highly accurate statistics, and Equatorial Guinea is an especially closed society), it is interesting that three of these six extra countries had populations of under one million in 1982: Mauritius, Swaziland, and Botswana. (Gabon and Gambia also have populations of less than a million, and Congo had until 1970 [United Nations, 1984].) Furthermore, it is clear that if the North African states were included on the seemingly important criterion of their being members of the Organization of African Unity (OAU), then far more coup countries would have been added. Coups occurred in Egypt in 1952, Algeria in 1965, and Libya in 1969 (*Keesing's Contemporary Archives*). The emphasis on the North African countries being crucially different from countries included in the study, on the grounds that they belong to the "Middle Eastern, Arab-Islamic culture complex whose military patterns differ from those of Black Africa" (Johnson et al., 1984, p. 636), is not very convincing anyway. If "military patterns" implies having military coups or not, these examples suggest that they cannot be so dissimilar. The northern regions of the black

African countries bordering the North African states are populated by Arabs. The importance of the Muslim religion in black Africa is also not lost on the Ugandans who suffered under Amin, nor is it underestimated in the reinforcing cleavages of tribe and religion which divide Nigeria, nor especially perhaps in Sudan, in the light of the recent coup.

In short, it is not surprising that Johnson et al.'s and Jackman's results should differ. Nor is it surprising that (as demonstrated in their Table 6) Johnson et al. should find the addition of the six countries to Jackman's data to be relatively more important in reducing the significance of Jackman's model (R^2 = 50%) than the addition of the seven years (R^2 = 59%). Contrary to Johnson et al.'s condemnation of Jackman's model, however, these results suggest that his model continued to perform well for seven years. Crucially for Johnson et al.'s model, the implication of this arbitrary sample is that though it may achieve a better "fit" for the cases which are included, it is extremely doubtful that they will have generated any theoretical implications for either coups in Africa or coups in general.

The proposition that the causes of coups are different in Africa (or a part of Africa) from elsewhere is not only a testable hypothesis, but one which cannot be conclusively demonstrated on purely African data, even where the levels of significance achieved by a model are very high. The opposed hypothesis, that the causes of coups are essentially similar wherever they occur, needs to be rejected. Coups are, after all, world-wide phenomena. Johnson et al.'s justification for concentrating on Africa—that "the states of black Africa share a continuing record of coup activity into the 1980s" (1984, p. 636)—is peculiarly weak. Coup activity continues elsewhere too, as even cursory reading of political events clearly demonstrates. What then is needed is an unbiased sample of countries which both have and have not had coups, both inside and outside Africa. The notion that the sample should only include third world countries because the universe of countries would include "many states of the first and second worlds where coups are not a theoretical or practical problem" is quite extraordinary. It is one thing not to acknowledge that Mauritius and Madagascar are not black African countries; it is quite another to ignore the successful coups in Greece (1967 and 1973); Portugal (1974); and Turkey (1960 and 1980); of the unsuccessful coup in Cyprus (1974); of the attempted coup in Spain (1981), and reported plots in 1980, 1981, and 1983; of the report of an attempted coup in Romania (1983); and last but not least, of General Jaruzelski's military intervention in Poland in 1981 (*Keesing's Contemporary Archives*). These coups and events may not have occurred in such large numbers as in Africa or Latin America, but being general phenomena they clearly invite general explanation. Applying Occam's Razor, if a general explanation can be found for all coups, it must be preferred to specific area explanations. The effects on significance scores of restricted variance in independent variables has been demonstrated elsewhere (O'Kane, 1983); in particular, the importance of economic variables such as GNP per capita are likely to be obscured when their possible full range is excluded.

Jackman and Johnson et al. alike, while recognizing Zolberg's (1968) contribution in stressing the similarities between the tropical African societies, scorn his theory for being unable to explain why coups occur in some countries and not others. This is to miss entirely a most valuable insight into the explanation for coups. Zolberg argues that though these countries in theory share conditions conducive to coups, in practice other conditions may even so restrict coups—recent independence (mentioned by Johnson et al. but used in a different context)—or promote them—coups engender other coups within one country and can also be contagious outside a country.[13] Clearly the value of the notion of coups encouraging other coups cannot be employed when the dependent variable is a composite measure of all coup events, for it would become pure tautology.

The general suggestion that there may be conditions that do not cause coups but may hinder them in spite of their necessary preconditions is, though, a view held elsewhere (Luttwak, 1969; O'Kane, 1981). Looking again at the six countries which Johnson et al. add, it seems reasonable to suppose that such dependent relationships with South Africa might hinder coups in spite of the existence of necessary underlying conditions, and that the army (or police force where there is no army) might also present an obstacle to coups where the officers are foreign (as was the case in 1970 in Botswana, Mauritius, Cameroon, Gambia, Malawi, Zambia, and Gabon [Dupuy and Blanchard, 1972]). The presence of foreign armoured divisions within a state might also have a similar hindering effect on coups (O'Kane, 1981). Furthermore, these are problems which occur outside as well as inside Africa. It is also clear that the model produced by Johnson et al. does not reflect specifically African phenomena.[14]

The variables included in Johnson et al.'s model themselves require further consideration than they are given. Particularly worrying is variable 3, "political pluralism, 1975." This is clearly a variable which is not independent of the dependent variable. Indeed, it seems to be tautological: countries which have military intervention scores for either 1960–82 or 1970–82 are likely not to have "political pluralism" in 1975, and indeed, those countries with high military intervention scores are more or less bound not to have political pluralism in 1975. It is surely this circularity which accounts for the very high level of significance which this variable achieves. It would be interesting to know what levels of explained variance variable 3 achieves on its own, and also what the regression minus variable 3 achieves. Jackman's argument in respect of pluralism does not seem to have been disproved, as Johnson et al. claim, and even if it had, it is surprising that Johnson et al. fail to appreciate the implication of variable 4, which clearly supports Jackman. The attraction of the value of this "pre-independence" variable clearly lies in its being a condition which predates military interventions.

The same concern about the direction of cause and effect also applies to the two army variables: military intervention may itself lead to increased expenditure on the military, increased use of the military to suppress opposition, and to changes in the relative predominance of a particular ethnic group in the army. Rather than offering an explanation for military intervention, the fact that variable 2, "army political centrality, c. 1965," is not significant in the 1970–1982 model suggests that it is military intervention that causes both increased expenditure on the military and increased use of the military to sanction opposition. Variable 1, "army cohesion, c. 1950," remains significant for both models, but it is not convincing from the explanation of this variable that it is measuring what it claims. Logically, it should be a measure for the proportion of the army composed of a single ethnic group. It seems more likely that it is a measure of the proportions within the population as a whole multiplied by the absolute size of the army, a variable closer to Jackman's view of the importance of ethnicity, and therefore at least as likely to be as supportive of his views as of Johnson et al.'s. (I wonder still how they have dealt with those countries having only an armed police force and no army; if they have been scored as zero, then it is certainy neither surprising nor of any importance to find that countries without armies do not have armies intervening.)

The remaining variables also cause worries about the theoretical importance of the findings. It is still possible that military intervention has had an effect on these socioeconomic variables. Sometimes the military has a good record of encouraging mobilization (e.g., Brazil, 1964). This could then account for the rather large decline in the R^2 from 87% to 68%, though the current view seems to be that the military's performance in government does not differ greatly from civilian performance (Jackman, 1976; McKinley and Cohan, 1975). Certainly the persuasiveness and interesting mixture of findings would have been heightened by the comparison of effect within the same period of time for all the variables. Given the large number of variables in the equation (10 + constant) and the smallness of the data set ($N = 35$), the possibility remains that some of these variables are significant only because they apply to a particular case, rather than indicate a general model as claimed. This is especially a concern given that none of the countries with populations of under one million have had a successful coup, and none of them are scored higher than 4 on the Total Military Involvement Score (TMIS). It would be interesting to see how Johnson et al.'s model performs on data from Jackman's 29 countries only. My expectation is that it would perform either little different from or less well than Jackman's model performed at 50% for the 35 countries. In short, Johnson et al. have not improved on Jackman. They have unintentionally directed attention away from the restricted analysis of African coups and towards the analysis of coups d'état as general phenomena.

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Professors O'Kane and Jackman direct their criticisms at our 1984 article in this *Review.* Given that the article in question is part of an ongoing research program involving a technical report (Orkand, 1983), several conference papers (Johnson and McGowan, 1985; Johnson and Slater, 1983; McGowan and Johnson, 1985a), and two other articles (McGowan and Johnson, 1984, 1985b), we must refer our critics and others interested in this exchange to these publications and available writings for answers to many of Jackman's and O'Kane's critical comments.[15] Restrictions of space force us to limit our response here to their more substantial criticisms.

Case Selection

Jackman and O'Kane are both troubled by the cases upon which we built our analysis of African military coups d'état. For Jackman, the issue of our case selection is crucial to his defense of his 1978 model: he claims that when we added 6 countries to his original set of 29, this resulted in "an inappropriate test of the validity of that model." O'Kane, in turn, states that we do not understand the assumption of ceteris paribus because we "have performed a test on a collection of countries that neither share the same backgrounds nor are randomly sampled." We find these argument to be unfounded.

We took care to spell out explicitly the justification for each of the 35 countries selected for analysis (Johnson, Slater, and McGowan, 1984, pp. 636–37). We note that neither Jackman (1978; see also Jackman and Boyd, 1979) nor O'Kane (1981, 1983) provided explanations for the cases studied in their research on coups d'état. We now learn that Jackman selected only those black African states independent as of 1965, and thus capable of experiencing a coup for "at least two-thirds of the period under consideration," and that four independent states were excluded because "they were not potentially capable of experiencing a coup for up to one-half of the period under consideration." Now that we know Jackman's decision rule, we agree with it; we used similar reasoning for selecting 1970 as our cutting point, so that all of the 35 states we studied were independent for at least 61% of our 1960–1982 time period (Johnson et al., 1984, p. 637).

Even within the limits of Jackman's shorter time frame (1960–1975), Botswana and Lesotho were independent for just over nine years, or somewhat more than 56% of the identified period; Swaziland and Mauritius were independent for over seven years, or more than 44% of Jackman's sixteen-year period. We do not understand, then, why Jackman excluded Botswana and Lesotho on the grounds that they were not independent for one-half his time frame, and would suggest, in regard to Swaziland and Mauritius, that 44% is sufficiently close to 50% that he should have studied these countries as well.[16]

When Jackman writes that we conducted an inappropriate test of his model because we included 6 states that "do not belong in the analysis," he forgets to mention that we did estimate his model with the Total Military Involvement Score (TMIS) for the period 1960–1982 as the dependent variable for his 29 states, and found it did not fit well (Johnson et al., 1984, p. 631, Table 6). Therefore, we cannot accept the conclusion that we conducted an inappropriate test. We evaluated his model both for his 29 cases and for our 35 cases, and in each instance it did not work, yet all cases met the test of Jackman's own decision rule: that they be capable of experiencing the phenomenon in question, coups d'état, for more than one-half the time period considered.

Jackman understands the issues involved here, for he has recently written that "Even a statistical generalization based on 50 cases can be very much affected by just one or two cases, and it is essential to be sure that such 'influential data points' are not responsible for the empirical results" (Jackman, 1985, p. 167). What we found and reported in 1984 is that the excellent empirical results he obtained in 1978 were highly sensitive to the addition of 6 new cases (thereby increasing *N* by 20%) and seven additional years of behavior. Moreover, we demonstrated that it was more the addition of countries than the increased time period that was responsible for the problems we identified (Johnson et al., 1984, pp. 630–31). We would suggest that we followed rather closely Jackman's then unpublished methodological advice concerning how generalizations in cross-national research can be affected by the addition or deletion of but a few cases.

O'Kane claims we do not understand the assumption of ceteris paribus because we "have performed a test on a collection of countries, neither sharing the same backgrounds nor randomly sampled." She objects to the inclusion of Ethiopia, Madagascar, Mauritius, Swaziland, Botswana, and Lesotho in the analysis, and defends Jackman's exclusion of these states. O'Kane argues that Jackman's 29 cases represent a homogeneous set with a theoretical justification, whereas our 35 African states represent an inappropriate mix of tropical, black African and "other" Third World states.

The logic of comparison does not mean that all cases should possess highly similar attributes—comparison inherently connotes differences as well as similarities (DeFelice, 1980). If O'Kane believes that only homogeneous subpopulations may appropriately be substituted for random samples of states, then she should consider Jackman's arguments that "to say that two items are comparable also may be taken to mean that they can be compared for similarities and differences . . . comparative analysis does *not* require that items being compared be of the same class. Such a restriction, in fact, unnecessarily reduces the range of variability that we seek to understand" (Jackman, 1985, p. 168).

If, as O'Kane claims, it is wrong for us to include Madagascar and Mauritius because they are manifestly not tropical or black African states, why is it appropriate to include Mauritania and the Sudan, as both Jackman and we did, given that these two countries are dominated by Moorish and Arab cultures quite as different from the black African cultural mainstream as are the predominantly Indian and Afro-Indonesian cultures of Mauritius and Madagascar? Why is it wrong for us to include the ethnically homogeneous states of Botswana (99%), Lesotho (99%), and Swaziland and not also wrong to include ethnically homogeneous Burundi (90%), Rwanda (90%), and Somalia (97%), as both we and Jackman did?[17] Jackman's 29 cases are, in fact, quite heterogeneous in terms of historical, cultural, and ethnic patterns, and no more meet O'Kane's conception of a ceteris paribus exercise than do our 35 states.

The presumption, apparently shared by Jackman and O'Kane, that we should study only those countries "sharing the same backgrounds" is self-defeating. While the member states of the Organization of African Unity (OAU) share many similarities, such as poverty, underdevelopment, dependence, and, with but two exceptions, a history of colonialism and recent decolonization, they also clearly manifest numerous and increasing differences (Tordoff, 1984, pp. 1–4). Any sizable subset of these states will contain striking and, no doubt, theoretically important differences. Ceteris paribus in the study of Africa is not an absolute, but rather a matter of degree; we remain satisfied that common membership in the African regional subsystem and in its major interstate organizations, such as the OAU and the African Development Bank, renders our set of 35 states meaningfully comparable.

O'Kane defends Jackman's (1978, p. 1268) exclusion of Ethiopia and criticizes us for including it because "Ethiopia, unlike all the other tropical African countries, was never colonized." We know that Ethiopia was never colonized by a European power, but only briefly occupied by the Italians. Liberia also was never a colony. No rule of logic supports excluding Ethiopia for lack of a colonial background while not also excluding Liberia for the same reason.[18] Yet both Jackman (1978, p. 1274) and we (Johnson et al., 1984, p. 627) have included Liberia, and O'Kane does not object.

Regarding the case of Ethiopia, we were puzzled to read Jackman's statement that ". . . the lack of political parties in Ethiopia, circa 1960, makes one wonder how they generated a party dominance score for this country." The clear implication is that we made up a score that did not exist. Our party dominance score for Ethiopia (15%) comes from the same source originally used by Jackman: Morrison, Mitchell, Paden, and Stevenson (1972, p. 103, Table 8.8).

Model Specification

Jackman attacks our model specification, claiming that we gave "little attention" to this critical theoretical exercise. But our 1984 *Review* article is but one part of a research effort that has been ongoing since October 1982. A technical report (Orkand, 1983) and one of our conference papers (Johnson and Slater, 1983) provided abundant explanation for our final, ten-variable model. Space constraints placed upon our *Review* article precluded a full discussion of model specification procedures beyond the literature review provided (Johnson et al., 1984, pp. 632–33), which did justify the model's theoretical underpinnings.

The technical report and conference paper (Orkand, 1983; Johnson and Slater, 1983) feature an extensive review of the literature on military intervention. They derive and specify from this literature five competing theoretical models of the causes of military involvement in African politics that deal with theories of political development, national economic performance, international economic performance, social mobilization and change, and military motivations (Orkand, 1983, pp. IV1–IV31; Johnson and Slater, 1983, pp. 6–35). Once specified, each model was evaluated against appropriate data for our 35 states (Orkand, 1983, pp. VI1–VI20; Johnson and Slater, 1983, pp. 43–55) in a fashion similar to O'Kane's (1983) strategy. The results of this stage in our research led us to conclude:

> This effort arrived at deductively specifying and estimating five models of military intervention in sub-Saharan Africa and it provides powerful evidence that a concept as elusive as military intervention can be statistically explained. . . . Clearly, it is plausible that a model which builds on the results of these five models might provide an even more powerful explanation. Theoretically, such a model is defensible because the individual models should not be viewed as mutually exclusive. Moreover, our statistical results suggest that none of the models can be completely ignored. Thus, a next research step is the combination of key variables into a more general model of military intervention. Such a model is the subject of subsequent research. (Johnson and Slater, 1983, pp. 55–56)

This subsequent research resulted in our published model, but only after we had thoroughly evaluated Jackman's (1978) model for its validity when it was applied to our research problem and expanded data base. While our *Review* article may give an impression of being "data-centric," our total research effort has been powerfully informed by theoretical reasoning.

Operationalization

Regarding our dependent variable, Professor O'Kane is critical of our focus on military coups, which she regards as "a highy dubious exercise." Her own research (O'Kane, 1981, pp. 297–98; 1983, p. 42) purports to examine "all" coups, whatever the nature of the participants, in "all" states ($N = 125$), during the period from 1950 to February 1971. She argues that our focus on African military coups caused us to overlook those instances where "the police may . . . be the important force in these African countries" or in which there was no overt use of force by the military.

We cannot have stated more explicitly (Johnson et al., 1984, p. 625) that we collected all coup events where "elements of the state's military, security, or *police* forces played a role in the event" (our emphasis). As previously noted (Orkand, 1983) and cited (McGowan and Johnson, 1984, p. 637), our conceptual and operational definitions of military coups d'état included the recognition that "physical injury resulting from a coup may be negligible or pronounced" (Orkand, 1983, p. A2). Both police actions and threats of violence are included in our conception of military coups.

In criticizing our research O'Kane does not sufficiently recognize that, at least in Africa, as Tordoff (1984, p. 153) has recently written, ". . . coups are typically army coups." Yet, in her own research she does recognize the significance of the military in such research: "the variable representing the presence of a well-organized army . . . was both significant and correlated in the expected direction, *suggesting military characteristics to be important for the event of a coup*" (O'Kane, 1983, p. 33, our emphasis). Having compared our coup data with her list of all countries where coups occurred between 1950 and 1971 (O'Kane, 1981, p. 298; 1983, p. 42), we find the same 14 African states as in our own list. Her more inclusive conception of coups d'état does not generate, in the African instance, more coup observations than our allegedly "narrower" definition.

Indeed, O'Kane (1981, p. 298, n. 37)

claims that of these 14 states, only 4—Benin, Sierra Leone, Sudan, and Togo—experienced more than one coup in the years 1950–1970. We agree in these four instances, but we have recorded multiple coups in another 5 states—Burundi (2), Congo (3), Nigeria (2), Uganda (2), and Zaire (2). Our definition of coups d'état led us to observe 27 coups in these 14 countries, whereas O'Kane counted only a maximum of 21 coups, and perhaps less.[19]

Finally, of the 28 coups d'état that occurred in sub-Saharan Africa in the period 1950–1971, the only one not part of our data set is that which took place in Lesotho in January 1970 (Morrison et al., 1972, p. 280), as it does not meet our stated criteria for a military coup. We note that while O'Kane states that "those events in Lesotho are often described as a coup" she did not count this event as a coup d'état in her own research (O'Kane, 1981, p. 298; 1983, p. 42). O'Kane's argument that not all coups are military coups may have some merit outside sub-Saharan Africa, but it is a distinction without an empirical difference as far as Africa is concerned.

Both Jackman and O'Kane criticize one of our key explanatory variables, political pluralism measured in 1975. Because our dependent variable, TMIS, is measured over the period from 1960 to 1982, Jackman believes that our operationalization leads to a simultaneity bias, while O'Kane finds this variable tautological and circular. Our critics appear to be saying the same thing with different words. Because they believe that political pluralism is conceptually and operationally very close to being the exact opposite of military intervention and coups, they believe the 1960–1975 TMIS may have generated the 1975 political pluralism scores, that in 1975 pluralism and TMIS are the same thing with different names, and that any impact 1975 pluralism has on the 1976–1982 TMIS is not causal but rather a function of circular reasoning and tautological measurement procedures.

First, it should be noted that our TMIS index includes attempted coups and reported coup plots as well as successful coups. Together they comprise 270 points, or 51% of the 530 points in our TMIS index for 1960–1982. Logically as well as empirically, African states can accumulate a considerable amount of our dependent variable while maintaining a civilian, pluralistic political regime.

Second, political pluralism and military coups are not the same thing either conceptually or operationally. Conceptually, pluralism refers to the degree to which individuals and groups can express and act upon their political beliefs and preferences. In Africa this is most widespread in multiparty civilian regimes and most restricted in entirely military regimes that rule by decree. One-party and one-party dominant regimes in which the military may or may not share power with civilians are intermediate. We operationalized this concept by using an index which gives the highest pluralism scores to regimes in which many political parties of equal size sit in a legislature, and the lowest value to regimes in which there is no legislative body at all. We are tautological only if successful military coups always lead to long-term African military regimes that rule by decree without permitting the existence of legislatures and political parties—but such is not the case.

Third, contrary to what Jackman asserts, even when African militaries seize power in successful coups d'état, they do not always completely destroy political pluralism. Jackman has stated elsewhere that "the bulk of the cross-national evidence . . . indicates that the military/civilian government distinction does not help account for patterns of economic growth, social change or even the size of military budgets" (Jackman, 1985, p. 170). He cites his own previous work (Jackman, 1976) in suggesting the lack of impact these types of regimes have on subsequent economic, societal, and military dynamics.

Through the end of 1984 there have been 56 coups in 26 of the 45 sub-Saharan African states (McGowan and Johnson, 1984, p. 638; 1985a, Tables 2 and 3). Only in 35 instances in 23 states did the military form a purely military regime which lasted any period of time. In 21 instances it quickly turned power over to a new set of civilian politicians, or formed what Anene (in progress) calls a "fused regime," in which power was shared with civilian politicians and technocrats. Even in the 35 cases where the military has ruled directly for a period of time, in only 11 countries did it continue to rule by the end of 1984. In the 24 other instances of military regimes since the first military coup in the Sudan in 1958, the military has turned power over to elected civilian regimes nine times and has formed fused civilian-military regimes in the remaining 15 cases (Anene, in progress). Clearly, as we argued (Johnson et al., 1984, p. 635), some degree of political pluralism, even if it is only of the one-party or one-party dominant type of regime, has been maintained by numerous African states (of the 19 regimes that have never had a coup, 18 are multi-party or one-party dominant and Swaziland is a monarchy with no parties) or has been reintroduced in the 15 states noted above with what Collier (1982, pp. 147–48) calls "one-party plebiscitary" regimes. Our argument is that, all other things being equal, serious military interventions are becoming less likely where such regimes are in place.

Fourth, we can report new evidence relevant to this point. Nine out of 45 sub-Saharan African states never witnessed an attempted or successful coup during the period 1958–1984. Three of these states have maintained multiparty regimes—Botswana, Lesotho, and Mauritius—while five have single-party civilian governments; Swaziland is a monarchy without parties (Young, 1984, pp. 468–69). Next, we can partition our data into two time periods, 1958–1975 and 1976–1984. If we then ask, for each time period, what type of political regime was in place in each of the remaining 36 states at the time of one of the 56 successful coups or one of the 62 unsuccessful coup attempts, two highly interesting patterns are evident. First, even though the initial time period covers 18 years and the second only 9 years, the 1976–1984 period includes 42% of all coups and coup attempts. Serious military interventions are increasing with time in sub-Saharan Africa (McGowan and Johnson, 1984, pp. 641–44). Second, and most importantly for this discussion, there has been a pronounced shift with time in the kind of regime experiencing serious interventions. In the period 1958–1975, fully 73% of serious military interventions happened to civilian regimes in which some or a great deal of political pluralism existed, thereby indicating a positive relationship between pluralism and interventions, as Jackman found in 1978 (p. 1271). Beginning in 1976, however, the pattern is reversed, with 64% of the interventions happening to regimes led by the military, thereby indicating a negative relationship between coups and pluralism, as we found in the context of a multiple regression analysis (Johnson et al., 1984, pp. 634–35). Furthermore, the explanation of this is not that there are no civilian regimes left in Africa against which the military can act. As of the end of 1984, there were 21 fully civilian regimes, 13 fused regimes, and only 11 entirely military regimes, a clear preponderance in the civilian, pluralist direction.

Fifth, if Jackman or O'Kane had tested their claim of "tautology," they would have found that the bivariate correlation between our dependent variable, TMIS 1960–1982, and our measure of political pluralism in 1975 is only −.42. This correlation is hardly of the magnitude to suggest tautological measurement. Moreover, four other bivariate correlations between our dependent variable and independent variables have similar or greater correlations: army cohesion, .49; pre-independence voter turnout, −.41; percentage of industrial jobs, .40; and army political centrality, .36.

Lastly, one cannot have it both ways. If we have introduced a simultaneity bias into our model because it includes a variable measured in 1975 along with a dependent variable measured for the period 1960–1982, what about Jackman's own model, which covers the years 1960–1975, and which contains as an indicator of social mobilization a variable measured in "c. 1965 to 1966" (Jackman, 1978, p. 1273)? Jackman simply assumes away the problem of simultaneity bias by remarking that this "is a variable that *probably* underwent little pronounced change in the preceding six years" (1978, p. 1273, our emphasis). While plausible, in this statement he assumes for himself what he asks us to demonstrate.

Model Validity

The comments made by Jackman and O'Kane primarily attempt to call into question the internal validity of our model, but without presenting alternative models demonstrably superior to it. We did demonstrate (Johnson et al., 1984, Table 4) the shortcomings of Jackman's model when we applied it to our more comprehensive data, and then presented what we continue to believe is a superior model.

O'Kane suggested a direct test of the competing models when she wrote "It would be very interesting to see how Johnson et al.'s model performs on data from Jackman's 29 countries only. My expectation is that it would perform either little different from or less well than Jackman's model performed at 50% for the 35 countries." Actually, Jackman's model, when fitted to our data, performed at the 43% level, with only one out of seven explanatory variables remaining significant (Johnson et al., 1984, p. 628). Table 1 undertakes this evaluation. The first column repeats our original 1984 result. Column two fits our model to Jackman's 29 cases. The statistical evidence indicates decisively that it performs at least as well on these 29 countries alone as it does when 35 cases are examined. Our parameter estimates are all of the same sign as before, and they evidence little change in magnitude. If our indicator of industrial job creation is now only significant at the .10 level—something Jackman (1978, p. 1271) was willing to accept for his interaction term for party dominance and participation—the total regression model explains slightly more variance than before.

Moreover, because we have continued to collect TMIS data beyond 1982, we can provide an additional test of our model's explanatory validity. During 1983 and 1984 our 35 states experienced 4 successful coups, 6 coup attempts, and 12 reported coup plots (McGowan and Johnson, 1985a, Tables 2 and 3). These events add a total of 50 TMIS points to our index, and represent two additional years of behavior. In column three of Table 1 we have fitted our original model to this temporally expanded data set, and it fits very well. Our results are stable and valid with respect to both time and space, and for our research problem our model is demonstrably superior to Jackman's.

This belief is based not just on our original analyses and the new regressions reported in Table 1, but also on our model's demonstrated utility as a forecasting tool. In an exercise in *ex post* forecasting (Orkand, 1983, pp. VI5–VI10), we used Jackman's model in a discriminant function analysis to assign our 35 African countries to "coup-prone" and "non-coup-prone" groups for the years from 1960 to 1982. Jackman's model mis-

Table 1. Criticism and Validity: Further Evidence

Variables	Our Previous Result[a]	O'Kane's Suggested Test[b]	Our New Result[b]
Dependent Variable	TMIS[c] 1960–82	TMIS 1960–82	TMIS 1960–84
Constant	79.708 (10.115)[d]	75.708 (13.658)	74.503 (11.237)
Army Cohesion, ca. 1970	.319 (.113)	.371 (.117)	.433 (.126)
Army political centrality, ca. 1965	.0037 (.0016)	.004 (.002)	.004 (.002)
Political pluralism, 1975	−76.312 (8.863)	−43.823 (16.610)	−75.610 (9.846)
Turnout, pre-independence	−17.986 (6.521)	−26.940 (8.639)	−18.354 (7.244)
Percentage of population in agriculture, 1960	−.653 (.095)	−.620 (.123)	−.600 (.106)
Percentage of increase in the population of the capital city, 1950–1960	.043 (.008)	.055 (.009)	.044 (.009)
Absolute increase in percentage of industrial jobs, 1960–1978	−180.339 (68.881)	−131.534* (74.598)	−118.802** (76.517)
Percentage increase in GNP, 1965-1970	−16.633 (4.228)	−16.133 (5.374)	−17.333 (4.697)
Ratio of exports-imports to GNP, 1965	−.763 (.137)	−.815 (.221)	− .710 (.512)
Percentage increase in export commodity concentration, 1960–1965	.150 (.034)	.143 (.035)	.174 (.038)
Number of Cases[e]	35	29	35
R Square	.906	.921	.900
$\bar{R}$ Square	.872	.884	.864
F	23.24	20.95	21.69
p of *F*	.0001	.0001	.0001

[a] Source: Johnson, Slater, and McGowan (1984, p. 635). Calculated by SAS PROC SYSREG, release 79.6, in March 1983.

[b] Calculated by SAS PROC SYSREG, release 82.3, in September 1985.

[c] Total Military Involvement Score.

[d] Standard errors are reported in parentheses.

[e] For the sub-Saharan African countries in each regression, see Johnson, et al. (1984, p. 627).

*Only significant at the .095 level.
**Only significant at the .134 level.

Table 2. ***Ex Ante*** **Forecasts of Serious Military Intervention Probabilities for 1985**

High Probability States (N=15)			
Burkina Faso	.99	Chad	.94
Ghana	.99	Mauritania	.94
Nigeria[a]	.99	Sudan[a]	.94
Benin	.98	Niger	.92
Burundi	.98	Zaire	.91
Congo	.98	Ethiopia	.89
Uganda[a]	.98	Sierra Leone	.88
Central African Republic	.97		
Average Probability = .952		Average TMIS[b] 1960–1984 = 26.9	
Moderate Probability States (N=11)			
Somalia	.73	Zambia	.50
Guinea	.67	Rwanda	.43
Togo	.66	Kenya	.38
Mali	.63	Senegal	.34
Madagascar	.58	Liberia	.30
Tanzania	.56		
Average Probability = .525		Average TMIS 1960–1984 = 10.3	
Low Probability States (N=9)			
Cameroon	.23	Malawi	.03
Gabon	.09	Gambia	.01
Swaziland	.07	Lesotho	.01
Botswana	.03	Mauritius	.01
Ivory Coast	.03		
Average Probability = .057		Average TMIS 1960–1984 = 1.8	

Source: Modified from McGowan and Johnson, 1985a, Table 5.

[a] *States where successful military coups have occurred between January 1, 1985 and September 20, 1985.*

[b] Total Military Involvement Score.

classified 20% of the cases, indicating that Botswana, Guinea, Lesotho, Malawi, Swaziland, and Tanzania were coup-prone, when in fact they did not experience coups in these years. Madagascar was misclassified as non-coup-prone, when in fact it had coups in 1972 and 1975 (note that Jackman's model did correctly classify two of our cases that he did not study, Ethiopia and Mauritius, and that Guinea, Malawi, and Tanzania were part of his original 29 states). When this exercise was repeated with our ten-variable model (Orkand, 1983, pp. VII1–VII7) only 8.6% of the cases were misclassified —Guinea and Zambia were said to be coup-prone and Liberia was non-coup-prone. This superior performance led us to undertake subsequent *ex ante* forecasts using our model.

As recently reported (McGowan and Johnson, 1985a, Tables 2 and 3), in March 1983, again using discriminant function analysis, we classified 35 sub-Saharan African states into a group of 16 coup-prone countries where we forecast that serious military intervention events were most likely during 1983 and 1984, and into a group of 19 non-coup-prone states where coups and coup attempts were relatively unlikely. As noted, we continued to collect military intervention events, and we observed 4 coups, 6 coup attempts, and 12 plots, representing a total of 50 TMIS points during these two years. Seventy-four percent of these points happened in our smaller group of coup-prone states. Even more significantly, eight serious events, comprising 3 coups and 5 attempts, occurred among our coup-prone group in Burkina Faso (formerly Upper Volta), Ghana, Nigeria, Sudan, Niger, and Mauritania. Only two events, a coup in Guinea, to which we gave a .51 probability, and an attempted coup in Cameroon, which we forecast as only .13 likely, happened in our larger non-coup-prone set of countries.

These favorable results encouraged us to generate in January 1985 *ex ante* forecasts for the current year (1985), given in Table 2. As reported elsewhere (McGowan and Johnson, 1985a, pp. 11–13), we used simple Bayesian techniques to transform our prior probabilities from 1983–1984 into posterior probabilities for 1985 based upon the historical experiences of all 35 states since 1960. As of 20 September 1985, coups d'état have taken place in the Sudan ($p=.94$), Uganda ($p=.98$), and Nigeria ($p=.99$). While we would be the first to point out that our probabilities should not be taken to represent point predictions, we would argue that our threefold classification into high, moderate, and low probability states does make a great deal of sense, and is predictively accurate—the coups that have so far occurred in 1985 have all been among our high probability states.

Conclusion

Following Hermann's (1967) classic discussion of model validation in international and comparative politics, we would conclude that it is incumbent upon Jackman and O'Kane to account for why our "misspecified," "best fit" model which ". . . contributes nothing to our understanding of the forces that underlie coups d'état" has proven to be both a powerful explanatory device and an efficient forecasting tool. We found many of the comments by Jackman and O'Kane interesting, and we thank them for taking our work so seriously. Nevertheless, we remain convinced that a model so adequate in accounting statistically for past behavior, and which also renders useful forecasts, clearly represents a contribution to our understanding of coups d'état and to the literature of comparative politics.

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Notes

*I would like to thank Mary Jackman and Brian Silver for their comments and criticism.

1. Johnson et al. (1984, pp. 636–37) suggest as a "rule of thumb" that countries with populations of less than one million be excluded from cross-national analyses. The rationale for this is unclear. Had they taken their own advice, three of these four countries that became independent after 1965 would also have to be excluded from their analysis. As of mid-1982, Botswana, Mauritius, and Swaziland each had estimated populations of less than one million (World Bank, 1984, p. 276).

2. Nor is it clear why total military manpower is the relevant quantity, since, all other things equal, one would expect this to vary directly with population size. To what extent, then, is total military manpower serving as a very crude proxy for population size, and why should the latter have a bearing on coups? When comparing the relative size of military organizations, the usual procedure is to calculate military participation ratios, which are formed by

dividing the total military manpower by the size of the population at risk of being enlisted or conscripted, namely, the number of working-age people.

3. Since army political centrality is a product, it also represents a statistical interaction between government sanctions and defense expenditures, with the additive effects of the latter two components again assumed (with no discussion) to be zero.

4. Johnson et al. (1984, p. 634, n. 12) report that they calculated fractionalization according to the formula in Bornschier and Heintz (1979, p. 267). The measure employed is that popularized by Rae and Taylor (1970) and earlier developed by Greenberg (1956). Problems with this measure of fragmentation are discussed by Jackman (1978, p. 1265, n. 5).

5. The logical issues involved here have been addressed in previous literature on the military in politics (see, e.g., Dowse, 1969).

6. There are two practical problems with the measure of urbanization (see, e.g., Taylor and Jodice, 1983, p. 162). First, what is the population size cutoff point that distinguishes urban from nonurban areas? It is difficult to resolve this issue in a satisfactory way, and the problem is compounded by the variety of national definitions of what constitutes a "city." Second is the even more difficult issue concerning the demarcation of city limits. However, Johnson et al. neither invoke these problems to justify their own approach, nor do they obviate them.

7. In their attempt to simulate coups based on this model, Johnson and McGowan select six countries for detailed analysis (1985, Table 2). For three (Ghana, Togo, and Uganda) the variable political pluralism had to be ignored because these states had no legislative body in 1975 (1985, n. 11).

8. In their Table 1, Johnson et al. report my estimates (col. 1) along with their own, using the same data (col. 2). The figures are identical, except that their estimates involving social mobilization and party dominance are 100 times larger than my own. Johnson et al. offer the following explanation for this discrepancy: "Jackman omitted decimal points from his percentage measures (sic) of social mobilization and party dominance, whereas we did not, which accounts for the different placement of the decimal points for the regression coefficients of these two variables in our two regressions" (1984, p. 625, n. 4). Johnson et al. seem unaware of the difference between percentages and proportions.

9. Johnson et al. consider the problem of potential collinearity in my model (pp. 629-630). This discussion is not fruitful, for at least two reasons. First, while it is true that my interaction terms are somewhat collinear with their components, it is also true that my parameter estimates all have acceptable t-ratios (see their Table 1, col. 1). Since the principal effect of collinearity is to inflate standard errors, it is evident that collinearity was not seriously leading me astray. Secondly, it is almost always true that collinearity is present when interaction terms are formed in the manner I followed, but other criteria (such as improved goodness of fit) are available to evaluate the importance of statistical interactions. The relevant arguments are addressed in greater detail by Friedrich (1982).

10. Recent developments in methods of event-history analysis seem to offer a particularly promising perspective for future analyses of coups (see Allison, 1984).

11. See Jackman (1978), O'Kane (1981), and First (1970).

12. This application to the military logically follows from Bachrach and Baratz (1962), where the argument is applied to decision making behind the scenes.

13. It is important to note the implication of the restricted data set for Jackman's model. Because he performs a *ceteris paribus* experiment, he cannot demonstrate the importance of conditions conducive to coups which are similar for all tropical African countries (as Zolberg suggests). As a consequence, Jackman cannot be sure about his claim to have estimated "a model of the structural determinants of coups d'état for the new states of black Africa in the years from 1960 through 1975." It is equally possible that he has simply discovered some additional obstacles to coups.

14. See O'Kane (1981) for an explicitly general theory concerned with dependency on primary exports, which produces a statistical model, and which, furthermore, explicitly tests for structural stability by calculating the F-Ratio (Johnston, 1963, pp. 136-37) for the two subsets of "developing" and "developed" nations and finds no significant difference.

15. Copies of the conference papers and technical report are available at cost from Johnson, while our data set is available from McGowan, again at cost.

16. Mauritius gained independence in March 1968. If we use 1968 as its first year of independence, then it was capable of experiencing coups for exactly 50% of Jackman's time span.

17. The percentages are the portion of the population sharing the same mother language, i.e., Tswana, Sotho, Rundi, Rwanda, and Somali (Morrison et al., 1972).

18. O'Kane seems to be unaware that Jackman excluded Ethiopia because he thought (incorrectly—see Morrison et al., 1972, pp. 102-3) that as a historical monarchy it did not have a history of electoral activity.

19. We observed 5 coups in Benin (formerly Dahomey) between 1963 and 1969 (see Morrison et al., 1972, pp. 231-32), and all we know from O'Kane is that Benin had at least 2 coups between independence in 1960 and the end of 1970. Her actual coup count may be as low as 18.

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THE IMPACT OF TAXES IN DEVELOPED CAPITALIST COUNTRIES

A research question of perennial interest is, "What are the exogenous variables which best account for the differential distribution of income across nations?" Better understanding of the causes of inequalities in income distribution has been sought by investigating the effects of tax policies on income allocation and growth in capitalist countries. The quantitative analyses of these matters have engendered some debate about the adequacy of data, and the sufficiency of measurement. A preliminary foray into this research vineyard by Claudio Katz, Vincent Mahler, and Michael Franz is criticized here by Ronald King and Steven Jackson. In turn, Katz, Mahler, and Franz respond.

In the current debate over the social, economic, and political determinants of income distribution across nations, surprisingly little attention has been given to the public policy instruments through which governments are alleged to make their mark. To this extent, the recent paper by Katz, Mahler, and Franz (1983) is an important contribution to the literature, examining the impact of taxes upon the growth and allocation of incomes in advanced capitalist countries. Nevertheless, that paper is liable to criticism. In a previous note in this *Review*, Przeworski and Wallerstein (1985) examined the issue of growth, suggesting a theoretical complexity in the relationship between marginal tax rates and business investment. In this comment, we wish to address the empirical issue of actual income redistribution. We will focus internally on the authors' specification of dependent and independent variables, and externally on their general research design.[1] The results from our reanalysis suggest modification of the optimistic conclusion by Katz et al. "that the fiscal strategies typically adopted by liberal and social democratic governments to distribute income more widely do have the intended effect"—i.e., that their higher utilization of progressive tax instruments is statistically "associated with relatively egalitarian patterns of income distribution."

Dependent Variable

Katz et al. take their dependent variable from Malcolm Sawyer's study (1976, pp. 3-36) for the Organization for Economic Cooperation and Development (OECD), which generated reasonably comparable income distribution data for a limited number of countries for some single year in the late 1960s or early 1970s. The post-tax standardized data set selected by the authors operationalizes income as factor

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VOL. 80 NO. 1 MARCH, 1986

earnings in labor and capital markets, plus public and private transfers, minus direct (but not indirect) taxes and social security contributions, all adjusted for cross-national demographic differences in household size. Surprisingly, this measure of post-tax income shares is used without comparison to the pre-tax original position. We thus prefer to use percentage change, pre- to post-tax, which is available from the Sawyer data in unstandardized form, and for 10 rather than 11 countries. Their measure possibly incorporates some negative feedback from taxation to market apportionment. Yet post-tax income, taken by itself, is the product of much more than tax redistribution effects, and thus it is suspect whether the authors are actually focusing upon the contributions specifically induced by government revenue mechanisms.

Regardless of the measure employed, the total amount of effective fiscal redistribution is empirically quite small. Calculated in terms of Gini coefficients (although the findings are not substantially altered by the use of any other conventional summary statistic), all Sawyer countries do experience some egalitarian improvement. The mean gain, however, is only 6.37%, and the correlation of post-tax to pre-tax distribution is .919. On the average, the bottom quintile of the population receives only 6.0% of income, even after government taxes and transfers, while the top quintile garners 41.0%. In just three countries (Japan, the Netherlands, Sweden) does the bottom quintile have a post-fisc income share greater than 7%, and the latter two countries drop below that threshold without the statistical adjustment for demography.

Independent Variable

For their independent variable, Katz et al. use the share of a country's total economic product seized through the main forms of government taxation. Ratios of the tax instrument to the Gross Domestic Product (GDP) were constructed to tap the redistribution that occurs when fiscal tools extract money amounts unevenly across population deciles, and the implicit counterfactual is the income distribution that would remain if any such tool were dropped from the public repertoire, leaving the funds collected in private hands. Although this is plausible, it is possible to go a bit further and decompose the tax instrument/GDP ratios into their two component segments—tax instrument/total taxes and total taxes/GDP—roughly separating the structure of a revenue system from its overall level. This permits us to distinguish countries with low tax collection levels yet high proportionate utilization of progressive tax instruments from those with high collection levels yet low progressive utilization. It also permits us to assess statistically the impact of national differences in tax structure, while controlling for the differences in state size. Such an analysis implies an alternative counterfactual, in which the size of the fiscal state is exogenously set by expenditure needs and macroeconomic objectives. Thus the redistributive effect of a given tax instrument is assessed relative to possible replacements.

The separation of tax structure from tax level produces some important findings. Only 5 of the Sawyer countries collected as much as half of their taxes from ostensibly progressive revenue sources (i.e., income, corporate, estate, and property taxes); only 7 of the 23 OECD countries did so in 1980. For the Sawyer sample, the correlation of total taxes/GDP with progressive taxes/total taxes is −.428, which contrasts with the positive association ($r=.525$) of tax state size with welfare transfers/total expenditures. Similarly, it is state size ($r=.583$), not relative reliance upon progressive tax sources ($r=.082$), that correlates with socialist ministers. These results are really not surprising.

Social security contributions are earmarked for transfer payments, implying a direct connection between regressive taxation and progressive welfare expenditures. Socialist governments rely upon income taxes a bit more than others, yet also upon regressive consumption taxes apparently necessary to raise needed revenues. High use of the property tax is principally a phenomenon of the English-speaking countries, while low corporate taxes are sometimes used by countries with low-unemployment fiscal policies to compensate business and encourage capital formation. The complexity of public revenue structures suggests that the authors' simple language of zero-sum redistribution is insufficient to capture the political logic of taxation policy.

Statistical Model

Advanced capitalist democracies, of course, do cause some redistribution through their revenue collection, although it must be noted that transfers apparently exert about double the impact of taxation. Our argument is that tax redistribution should be presented as a function of the original pre-tax position, the progressivity of the revenue structure, and the total size of the tax state. Translated into a formal model, the equation reads:

$$\%\text{ChangeDist}[\text{Pre-Post/Pre}] = b_0(\text{ProgTax/Total Tax})^{b1} \times (\text{Total Tax/GDP})^{b2},$$

which contrasts with the much simpler formulation of Katz et al.:

$$\text{PostTaxDist} = b_3 + b_4(\text{ProgTax/GDP}).$$

Ideally, our multiplicative model could be estimated using logarithms. Unfortunately, with only 10 cases, and with distinct outliers among them, it is impossible to evaluate such a multivariate regression equation meaningfully. Bivariate correlation shows that the percentage Gini improvement, pre- to post-tax, is strongly associated with progressive taxes/GDP (r=.604), as Katz et al. would predict. Upon disaggregation, however, effective redistribution retains a significant relationship with state size (r=.513), while that for progressive tax share virtually disappears (r=.123). Table 1 shows that among the countries examined, only Sweden combined a high tax level with high progressive tax reliance. Importantly, Norway and the Netherlands, with below-median progressive tax reliance but large state size, also rank near the very top of the effective tax redistribution list. These countries all stand above the United Kingdom and Canada, which have high progressive tax reliance and median range state size, and far above the United States, Japan, and Australia, which have high progressive tax reliance but below-median state size. Germany and France, which have very low progressive tax reliance, also rank low in redistribution. The implication is that Gini improvement in the Sawyer sample is largely the effect of high levels of tax extraction by countries with average tax structures, rather than the effect of any extraordinary employment of ostensibly progressive instruments.

Graphic representations reveal the basis for the observed correlation differences. In addition, residuals were examined to see whether there is a progressive share impact that would otherwise remain obscured. A plot of progressive tax reliance against the redistribution residual not explained by the regression of Gini change with total taxes shows no visible relationship, and the insignificant correlation (r=.399) approaches zero upon the elimination of France, an extreme outlier. This analysis was then repeated for each major type of tax instrument. Redistribution might be affected by some of the parts, even if not by the package taken as a whole. When state size is controlled for,

Table 1. Gini Coefficients, Tax Redistribution, and Revenue Instruments

Country	Year	Pre-Tax Gini	Post-Tax Gini	Percent Gini Change[a]	Percent Progressive Tax/ Total Taxes[b]	Percent Total Taxes/ GDP[b]
Norway	1970	.354	.307	13.28	40.92 (.84)	39.19 (1.29)
Sweden	1972	.346	.302	12.72	51.56 (1.17)	42.79 (1.36)
Netherlands	1967	.385	.354	8.05	39.96 (.94)	38.05 (1.32)
United Kingdom	1973	.344	.318	7.56	54.61 (1.23)	31.87 (1.01)
Canada	1969	.382	.354	7.33	56.73 (1.32)	32.03 (1.07)
United States	1972	.404	.381	5.69	60.25 (1.37)	29.63 (.95)
Japan	1969	.335	.316	5.67	49.53 (1.15)	18.19 (.61)
Germany	1973	.396	.383	3.28	38.85 (.87)	36.27 (1.15)
France	1970	.416	.414	0.48	21.78 (.50)	35.58 (1.17)
Australia	1966–1967	.313	.312	0.32	62.78 (1.48)	24.82 (.86)

[a] $\dfrac{\text{Pre-tax Gini} - \text{Post-tax Gini}}{\text{Pre-tax Gini}}$

[b] Absolute percentage and ratio to OECD mean (in parentheses) for appropriate year. Sources: Sawyer (1976); Organization for Economic Cooperation and Development (1982).

correlations of instrument reliance with Gini change residuals all showed the expected sign—personal income, profits, and property taxes were positive; social security and consumption taxes were negative—but only the income tax result approached statistical significance ($r=.481$). Again, this association was entirely caused by the exceptional position of France. With France excluded, no relationship remains ($r=.042$).

Tax redistribution is the joint product of tax level and tax structure. That joint product is somewhat positive for all the countries in the limited Sawyer sample, although there are notable differences among them. From our evidence, we cannot safely infer that those differences in redistribution are influenced by differences in the way those countries tax, but we can assert that they are strongly influenced by the amount of taxes those countries collect. One must be wary when making multivariate inferences from a small and irregular sample. To the extent our findings are true, however, egalitarians should generally be distressed by recent neoconservative attacks upon the size of the state and the amount of taxes extracted, but they should also maintain doubt as to whether mere manipulations of public finance, as evidenced by the current agenda of "liberal and social democratic governments," can be sufficient to achieve desired ends.

Project Assumption

Finally, it must be remembered that the entire enterprise of Katz et al. (and these emendations as well) rests upon the critical assumption that greater use of progressive tax sources, relative either to GDP or to total taxes, corresponds proportionately to greater effective progressivity, both within each country's tax system and comparatively across them. Given the complexity and the cross-national differences in revenue codes, grounds for suspicion exist. On the basis of our model, once supplied with the pre-tax Gini distribution, the amount of post-tax redistribution, and the total amount of taxes extracted, it is logically possible to deduce effective tax structure progressivity. This has been performed by Tachibanaki (1981). Importantly, the correlation of his deduced progressivity coefficient with the progressive taxes/total taxes ratio is only .486; with the progressive taxes/GDP ratio (the Katz et al. indicator) it is .346; and it is lower still for each tax instrument taken individually. Progressive tax reliance is apparently not the same as tax progressivity, and provides a very poor measure for it, casting doubt upon the foundations of the authors' project design.

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The central point at issue in King and Jackson's comment has to do with our independent variable, tax instrument/GDP. Although they concede that our variable is "plausible," they prefer to decompose it into two components, tax instrument/total taxes and total taxes/GDP, thus allowing them to "distinguish countries with low tax collection levels yet high proportionate utilization of progressive tax instruments from those with high collection levels yet low progressive utilization." When they reanalyze OECD countries on this basis, they find it is the level of taxes, rather than the relative utilization of progressive tax instruments, that is associated with income equality, thus casting doubt on our "optimistic conclusion" that reliance on personal income taxes is associated with a relatively egalitarian income distribution.

In responding, we begin by looking closely at the proposed alternative variable, the tax instrument/total taxes ratio. We see this variable as a very narrow indicator of the relative tax burden across countries, and one not at all appropriate to the study of the relationship between taxes and income distribution. Only the level of a given tax instrument—tax instrument/GDP, the variable we use—can demonstrate the effects of a tax on any income group, since only this indicator measures the actual amount of wealth extracted from individuals and firms, and, concomitantly, the extent of the inducement to alter their economic behavior. The alternative indicator, tax instrument/total taxes, does not really do this: across countries, all it measures is whether a particular tax mechanism is more or less prominent as a proportion of overall tax revenues. Clearly, in a country characterized by a high progressive taxes/total taxes ratio but a small government sector, taxes could have only a small impact on income distribution, because whatever the internal structure of such a country's tax system, its taxes are low in absolute terms. Conversely, in a country with a lower progressive taxes/total taxes ratio, but much higher taxes overall, the burden of progressive taxes might be greater in absolute terms than in the first country, even if such taxes did represent a smaller share of a larger tax state.

To use an admittedly extreme hypothetical example, consider a country that relied on progressive taxes for 80% of its revenues, but whose total taxes represented only 5% of GDP: its progressive taxes/total taxes ratio would be 80%. Suppose that another country relied on progressive taxes for only 25% of its revenues, but that its total tax revenues represented 40% of GDP: its progressive taxes/total taxes ratio would be only 25%. However, the amount of revenue actually raised by progressive taxes—the figure we tap—would represent only 4% of GDP in the former case, but 10% in the latter. The second figure would seem to be the only one that reflected the impact of taxes on income distribution, since it represents the actual amount of income subject to progressive taxes. Or, to use the actual examples cited in King and Jackson's comment, the tax codes of the United States, Japan, and Australia are relatively progressive internally (in the sense of the tax type/total taxes ratio), since they rely heavily on income taxes, but the commitment to progressivity in these countries is shallow: taxes in general, including progressive ones, have a limited impact on income. By contrast, those countries with internally less progressive tax systems (in the tax type/total taxes sense) but high overall tax levels—Norway and the Netherlands in the examples given—would in fact be demonstrating a greater commitment to income distribution, at least to the extent that their progressive taxes raised greater amounts of revenue and had a greater impact on the income of the upper classes. In light of these considerations, it is not at all surprising that the tax type/total taxes and total taxes/GDP ratios are negatively related; nor is it surprising that overall tax levels, rather than tax progressivity (in the sense of the comment), are associated with relatively equal distribution. The latter finding, indeed, confirms our general conclusion that the size of the tax state, particularly that associated with personal income taxes, is associated with relative income equality.

In large measure, the argument at issue is one of semantics. This is particularly true when the comment criticizes us for focusing primarily on the level of taxes rather than on the internal progressivity of tax systems. This is indeed what we do, and our intentions on this point are very clear throughout the article. Thus, we begin our analysis by considering the level of all taxes as a proportion of GDP (Katz, Mahler, and Franz, 1983, p. 875)—what King and Jackson term "state size." We then go on to argue that this rather imprecise indicator might profitably be decomposed into its major constituent parts: taxes on income; profits and capital gains by individuals and corporations; social security contributions; property taxes; and taxes on goods and services. We find that the overall modestly positive association of all taxes/GDP and an egalitarian income distribution masks a fairly strong relationship between personal income taxes and an egalitarian distribution, coupled with no significant relationship in either direction between a relatively egalitarian distribution and most other tax instruments. We note that this is in part a function of the fact that personal income taxes are often very prominent in the tax state of the countries in question (see Katz et al., 1983, footnote 5). Further, we emphasize that other tax mechanisms, although potentially very progressive in themselves, are simply so limited in absolute terms that they cannot be expected to have much effect: for example, estate, inheritance, and gift taxes, and taxes on net wealth (Katz et al., 1983, p. 876).

King and Jackson also question our use of the dependent variable, post-tax income shares standardized for household size, as assembled by Sawyer (1976). They regard it as "surprising" that this measure was used without reference to pre-tax income shares, and note their preference for figures based on the percentage change, pre- to post-tax, of income shares.

We would agree that this proposed alternative formula indeed assesses the distributive impact of taxes in an immediate and formal sense: the difference between pre- and post-tax income shares represents by definition the discrete effect of taxation on various groups' income shares. What this indictor does not tap, however, is any impact a particular tax may have had on the economic behavior of individuals or firms which may in turn have affected their status with respect to distribution. These wider effects of taxation, rather than immediate distributive consequences, have been at the core of the current debate on the impact of taxes on distribution and growth. Conservatives, for example, have argued that while the use of progressive taxes to finance the welfare state may be immediately redistributive, it is in the long run counterproductive. Progressive taxes, in their view, impose on upper income groups a disincentive to save and invest, inhibiting economic growth and thus increasing unemployment among the poor, while welfare payments create a climate of dependency and complacency that ultimately erodes the standing of low-income groups. Liberals, of course, have disagreed, arguing that the ability of the state to shelter groups from the unbridled vicissitudes of the market is a central component of sustained economic growth that extends to all income groups (see Katz et al., 1983, p. 875). These wider repercussions have been at the core of recent debates on tax policies, with much less attention devoted to the immediate impact of taxes on income distribution. Thus, when King and Jackson make the point that "post-tax income, taken by itself, is a product of much more than tax redistribution effects," we respond that our variable has advantages for this very reason.

Finally, we note that there appear to be some rather serious technical problems with constructing an indicator of pre- to post-tax percentage change in income shares from Sawyer's data, problems which would render any study based on such an indicator questionable. For one thing, Sawyer explicitly cautions that "the data which are presented cannot be used for assessing the degree of progressivity of various countries' tax systems" (1976, p. 13), and himself refrains from directly relating pre- to post-tax income share figures. This is primarily because data unavailability and other problems with the figures for a number of countries forced Sawyer to derive one of the distributions, pre- or post-tax, from the other, making an indicator derived from the difference between them problematic. A further problem is that the pre-tax figures used by King and Jackson are not adjusted for household size. As Sawyer notes, "it is well known that the income of one-person households tends to be much lower than that of larger households," with the result that a country that "has a wide dispersion in the size of households will, *ceteris paribus*, show a larger degree of inequality" (1976, p. 18). The potential seriousness of this problem is suggested by the fact that Sawyer's adjusted and unadjusted post-tax figures are strikingly different for several countries (compare Table 3 with Table 10).[2]

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Notes

1. King and Jackson would like to thank Lynn Marshall, Jon Sax, and Marcia Stairman for research assistance; James Alt, Alex Hicks, Vincent Mahler, and Michael Wallerstein for helpful comments and suggestions; and both the Hatfield Fund for Economic Education and the Russell Sage Foundation for their generosity.

2. King and Jackson report that their reanalysis—based on their alternative indicator, progressive/total taxes—was dominated by the extreme outlier, France. This problem does not, however, affect our findings. As we reported (1983, footnote 12), an examination of Cook's distances (an indicator of influential cases in regression) revealed that for none of our findings was Cook's distance statistically significant at the .10 level. For France, the lowest significance level for Cook's distance in any equation linking personal income taxes to income shares was .63.

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RESEARCH NOTES

POWER SEEKERS ARE DIFFERENT: FURTHER BIOCHEMICAL EVIDENCE

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In an article previously published in this Review *I demonstrated that power-seeking, by which I mean the pursuit of social dominance, has a biochemical marker, namely, whole blood serotonin (WBS). Those individuals who are especially hard-charging and competitive have especially high WBS levels. This paper presents the results from an investigation of dynamics in the response of high WBS individuals to actual competition. My expectation—that they would exhibit special physiological activation in the face of challenge—is fully borne out by the evidence. Four hormonal indicators of activation were followed in blood samples taken during a series of social competitions. All four show distinctive patterns for the high WBS group. These results, in addition to providing new evidence on the behavior of the endocrine system in competitive settings, impressively support the view that WBS is a biological property having fundamental significance for behavioral political science.*

Those human beings who gain power or dominance over others have long been of keen interest to students of political life. Yet for all of the attention, scientific knowledge about *homo politicus* remains scanty. Little is known about the inner drive of those who devote themselves to the capture and use of power roles in social arrangements.

This report comes from a research program addressing that void. In a paper published earlier (Madsen, 1985) I presented evidence that the pursuit of social dominance has a biochemical marker, namely, whole blood serotonin (WBS).[1] Those individuals who in questionnaire responses showed themselves to be especially hard-charging and competitive—"Type A," in the jargon of medical scientists—were very likely to have high WBS levels. The multiple correlation of defining questionnaire items with WBS reached .70. As noted in that paper, this discovery echoed similar findings in a species of subhuman primates, where dominance was marked by high WBS levels (see Raleigh and McGuire, 1980; McGuire et al., 1982; McGuire et al., 1983).

It was clear from the outset that questionnaire data were less than ideal for my investigation. An obvious, and indeed crucial, concern was validation: How would high WBS individuals react to actual rather than merely remembered rivalry? That concern is addressed in the analysis reported here. In what follows, high WBS subjects are compared with *normals* in their responses to a series of competitions. The measured responses are physiological (and, it is important to note, involuntary). Four blood-borne hormones, each intimately involved in the body's preparation for action, have been followed in timed blood samples taken while the competition unfolded. The results show plainly that high WBS subjects are distinctive in their response to this challenge. Actual competition does indeed have a remarkable effect upon these individuals.

Methods

The present research was launched for the purpose of examining the relationship between psychological stress levels and distributions of influence in small social units processing problems of collective importance. Concern with serotonin level and its implications came later. Research design and methodological details are fully described in the earlier report from this project (Madsen, 1985). However, the following points bear repeating: the experimental subjects were 72 young, healthy males; they were processed through the competition in groups of six; the competition itself consisted of a series of logic puzzles that were to be solved collectively; sessions were all given at the same time of day but on separate days; and all sessions were conducted at a university hospital with trained medical staff doing all physiological procedures.

Eleven timed samples of venous blood were collected from each subject. The initial sample was taken when a subject arrived at the hospital, about 90 minutes before the competition began. Starting about 50 minutes later (after a quiet period and a standard dinner), a sample was taken every 20 minutes until 20 minutes after the competition ended.[2] All samples were subdivided, frozen, and maintained at −80°C until assays of their hormonal content could be undertaken. (The methods used in all assays are available upon request.)

Recall that in keeping with the earlier finding, whole blood serotonin values are used to define power orientation. Thus, the six cases with very high WBS levels (greater than 209 ng/ml, with a mean of 251 ng/ml) were separated out as strongly competitive, dominance-seeking individuals. They were to be compared primarily with a normals group ($N=55$; WBS from 90–200 ng/ml, with a mean of 144), but also with a low serotonin group ($N=7$; WBS from 40–82 ng/ml, with a mean of 72).

Four endocrine responses were followed, each one intimately related to the complex process through which the human body prepares itself to cope with threat or crisis. A few prefatory remarks about that process and the four hormones being followed are in order.

For human beings, the onset of stress, physical or psychological, brings forth what has aptly been called an endocrine cascade. Moreover, it is a cascade with elaborate feedback mechanisms, which in complex interaction regulate the extent of the endocrine response to that stress (Axelrod and Reisine, 1984). Prominent roles in this process are played by the hormones adrenocorticotropin (ACTH), cortisol, epinephrine, and norepinephrine.

ACTH is released from the anterior lobe of the pituitary gland, located at the base of the brain and triggered by a signal from the hypothalamus. The ACTH travels via the bloodstream to the adrenal glands (situated near the kidneys, one on each side of the body), where it causes in the outer layers of these glands (the adrenal cortex) the formation and release of a group of hormones called glucocorticoids, among them cortisol. Cortisol has a half-life of about 70 minutes. Along with the other corticoids, it exerts an important influence on metabolic processes, blood pressure, brain function, and, in general, on getting the body ready for action. This system is called the pituitary–adrenal cortical system, and its response to stress is neither as quick nor as transitory as it is that of the system to be considered next.

Also important in the human readiness reaction are the hormones epinephrine (E) and norepinephrine (NE), both classified as catecholamines and both a part of the sympathetic-adrenal medullary system. With the activation of this system by sympathetic nerves, E and, to a lesser degree, NE are secreted from the adrenals' inner portion, an entirely separate gland called the adrenal medulla. (NE is also secreted directly by sympathetic nerves, for which it serves as a transmitted substance.) The system responds to stress almost instantaneously, E and NE both flowing into the bloodstream. The hormones are quickly destroyed as well: the half-life for E is just over 1 minute, and that for NE about 2.5 minutes. With this quick on-off feature, the system is crucial to mobilizing the body for action in acute (as opposed to prolonged) challenge situations.

Although ACTH, cortisol, E, and NE are all involved in the stress response system, they do not all respond to an environmentally posed challenge in the same degree or at the same rate. In fact, there is still considerable uncertainty about the exact behavior of these four hormones in the face of such a challenge. That being the case, my predictions of their behavior in the present study could only be very general.

In the few relevant studies of plasma catecholamines there is evidence that psychological stress of the kind experienced when facing public speaking or mental arithmetic assignments produces a much stronger E than NE response (Dimsdale and Moss, 1980; Leblanc et al., 1979; Ward, Mefford et al., 1983) and does so especially among Type A individuals (Glass et al., 1980; Ward, Parker et al., 1983). The challenge presented by the competition designed for this study was psychological, but also was very different from the stressors used in previous work. Nonetheless, it seemed that I should find similar results if the high WBS individuals were specially activated by social rivalry. Hence, I expected to see marked elevation of E levels, and a more muted elevation of NE levels.

In contrast with the catecholamines, ACTH and cortisol have been the object of numerous plasma studies in humans. Few of these studies, however, look at the type of competitive challenge posed here, and none look at the WBS connection. However, again there is some important work on vervets to consider. McGuire, Raleigh, and Brammer (in press) have found that in groups from which the dominant male has been removed, cortisol levels were highest for those males which eventually emerged as dominant. Recall that male dominance here is accompanied by elevated WBS levels. This study, which came to my attention after my data already had been analyzed, suggests what I had in fact expected to find in those data: that high WBS individuals would show special elevation of cortisol levels. And since ACTH is a part of the same system, I anticipated similar results for that hormone, though perhaps with an earlier peak.

Measurement of the hormonal responses to environmental stimuli in the study proved to be anything but simple. Both ACTH and cortisol are subject to sizeable daily—i.e., circadian—rhythms, with zeniths coming roughly between 0700 hours and 0900 hours and nadirs roughly between 0100 hours and 0300 hours for normal subjects who adhere to a routine of nocturnal sleep and daytime activity. What I did not know when designing my study was that in the time of day it covered, roughly from 1600 hours to 2100 hours for each session, both hormones normally are falling rather rapidly. Thus, to measure the stress effect on ACTH and cortisol in my subjects, I could not assume a stable baseline, but had to establish the baseline for my comparisons empirically. Fortunately, after another complication discussed below, that baseline could be reasonably estimated from 24-hour data on healthy subjects collected in the Clinical Research Center of the University of Iowa Hospitals and generously made available to me by its director.

There also is some preliminary evidence

of circadian rhythms in the catecholamines (Linsell et al., 1985). However, in the case of NE the evidence suggests only a slight downward trend during the period in which I took samples. And in the case of E, though the downward trend in the latter part of that period is more noticeable, the really striking finding is the presence of very substantial variance around that trend line during the waking hours. Much more work needs to be done to decipher these curious patterns. Lacking any baseline data, I could only proceed with possible circadian effects in mind.

Further complication came from the discovery of a seasonal rhythm in the plasma ACTH and cortisol for my subjects. Returning to the Clinical Research Center's data, which happened to have been collected over the course of nine months, I found and measured the effect of seasonality on this group's cortisols after the effects of age and sex, which also turn out to be influential factors, had been removed. (This finding in 24-hour plasma cortisol data is new and will be discussed fully elsewhere.) The measured seasonality effect was then used to build a correction into the cortisol data for my subjects.

It was now possible to examine the responses of the entire set of subjects to the challenge imposed by the puzzle competition. The evidence is presented in Figure 1 for cortisol and ACTH, and in Figure 2 for E and NE. In Figure 1, the metric is percent above the healthy patients' baseline values. In Figure 2, the metric is percent above the minimum value in the 10-sample sequence. (The minimum value here may still be well above normal for the individual involved, but it is the only available estimate of such a baseline.) In these figures, and in those which follow, the plotted numbers are median values. Medians are used because the distributions are invariably skewed.

In Figure 1, note first that neither cortisol nor ACTH gets close to baseline levels in the 10-sample sequence. Cortisol does not drop below a 15% elevation;

Figure 1. ACTH and Cortisol Patterns

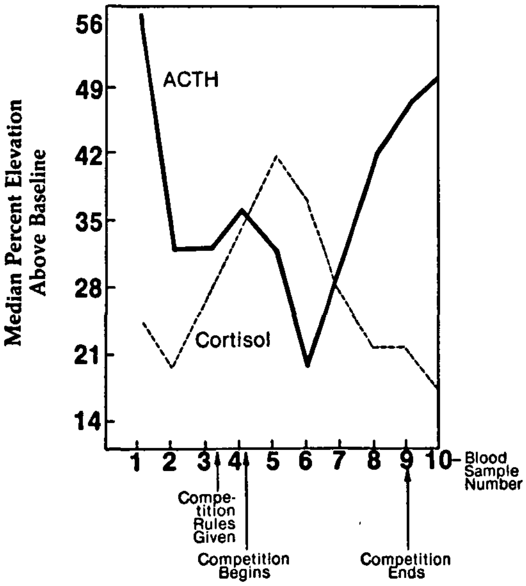
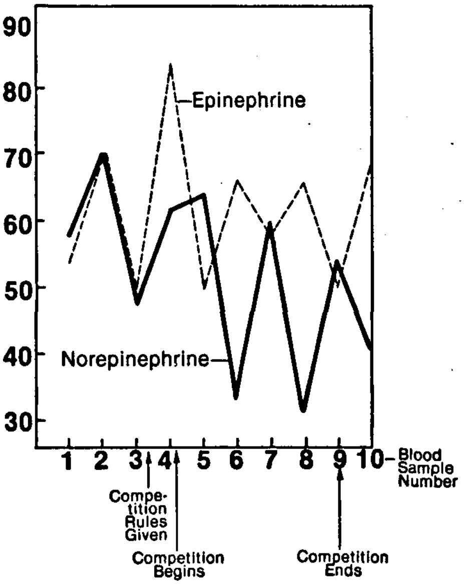


Figure 2. Epinephrine and Norepinephrine Patterns



ACTH does not drop below a 19% elevation. Even allowing for inexactness in baseline estimation, it is clear that on average these individuals were responding to the competitive context.

The contrast between the two patterns was a surprise. ACTH is highest at the time of venipuncture (when the first sample was taken)—perhaps not surprising given the fear many people have of this procedure. But median cortisol was not close to its highest point then; in fact, it was not far above its low. Cortisol climbed markedly after instructions for the competition were given, and reached its peak after the first two puzzles had been addressed. Thereafter it began a gradual fall. However, just as cortisol was falling, ACTH was climbing, ending the series not very far below the peak value registered with the first measurement. These results, almost mirror images of one another, mean that the point made earlier must be expanded. Not only may hormones in the same stress response system respond to challenge in different degree and at different rates; they may actually respond in different directions. Obviously, my simple assumption about the ACTH pattern was in need of revision.

In Figure 2 we also find substantial elevation of endocrine values. However, there is much more jerkiness to the trend lines for the catecholamines, which perhaps is to be expected with such short-lived substances. Were lines fitted to these greatly fluctuating data points, that for NE would slope downward (perhaps a circadian decline), whereas that for E would be almost flat. But the differences between these two catecholamine responses are more dramatic than that. On the whole, one is struck by the extent to which the two are completely out of phase.

It is plain that these two figures show mysterious differences in the behaviors of the separate elements of the stress response system, but it is equally plain that all four trend lines show significant acti-

Figure 3. Cortisol Patterns for WBS Groups

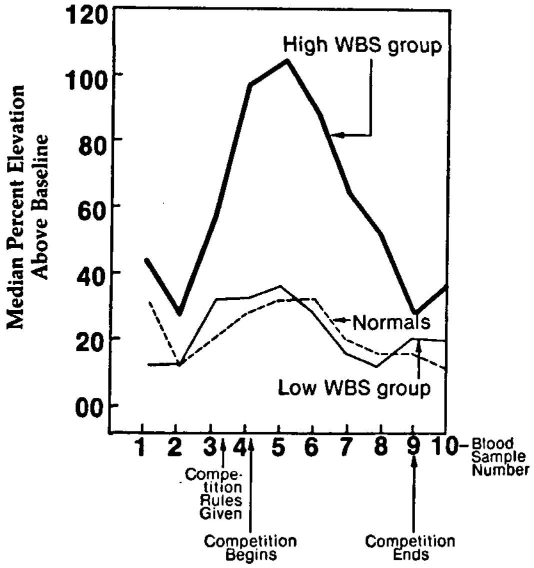


Figure 4. ACTH Patterns for WBS Groups

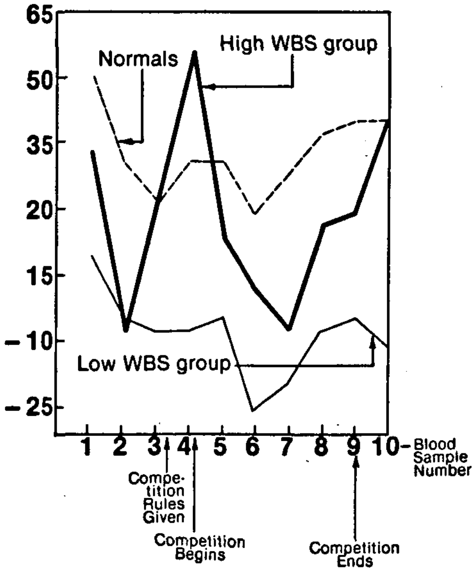


Figure 5. Epinephrine Patterns for WBS Groups

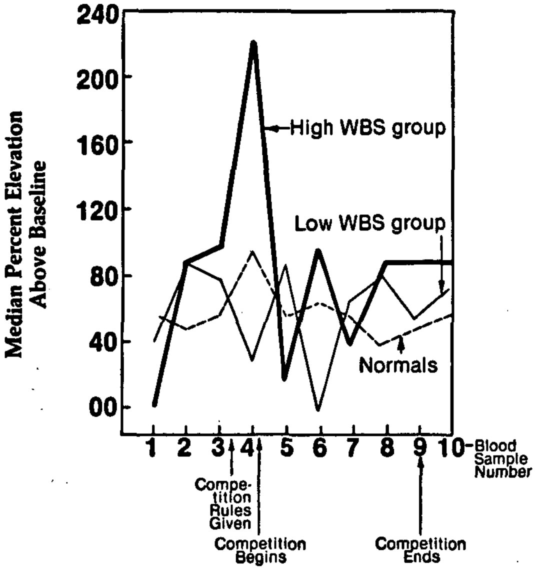


Figure 6. Smoothed Epinephrine Patterns for WBS Groups

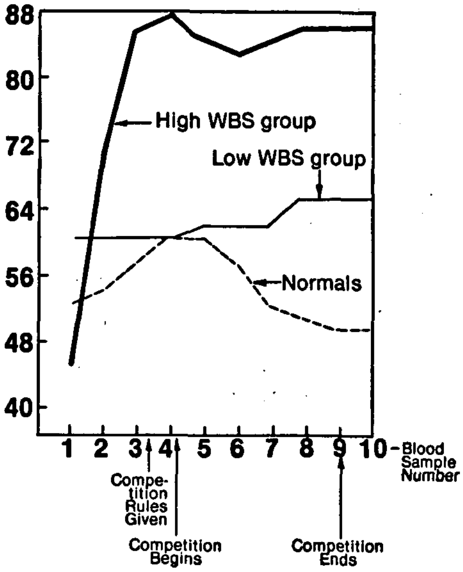


Figure 7. Norepinephrine Patterns for WBS Groups

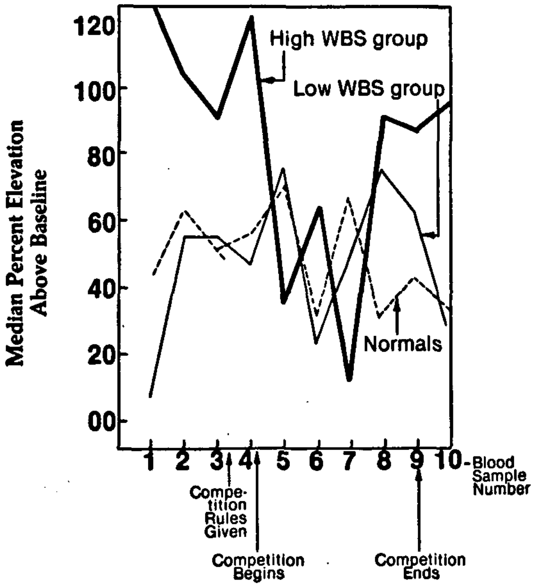
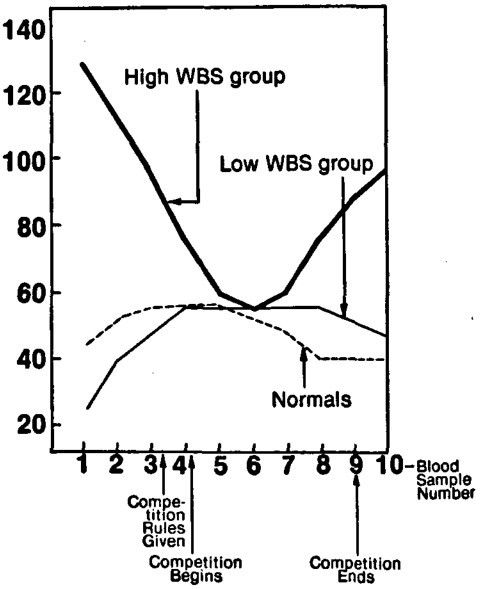


Figure 8. Smoothed Norepinephrine Patterns for WBS Groups



vation of the adrenal mechanisms. These subjects, on average, appear readied for a challenging situation. But were some more ready than others? It is to that question we now turn.

Findings

Hormonal responses for all three of the WBS groups are presented in each of the figures which follow. Figure 3 presents the cortisol series, Figure 4 the ACTH series. Figures 5 and 6 present two views of the E series, and Figures 7 and 8 two views of the NE series. Again, what is sought in these figures is evidence of a special activation for the high WBS group.

Figure 3 presents a clear and dramatic finding. The average cortisol values for the high-WBS group soar with the onset of competition. The peak, coming in the first sample taken after the competition had begun, shows just over a 100% increase. Neither the normals nor the low-WBS group show any such effect, although both show small elevations in the early stages of the competition and then a decline.

Figure 4, presenting the ACTH series, is puzzling. The high WBS subjects do not look here at all like they did in the cortisol series. In most instances their values, though above baseline, are not as high as those for the normals. What is distinctive for this group is the extraordinary variance in the series. The great peak, coming right after the nature and rules of the competition have been spelled out, is intriguing, as is the sharp drop for the main part of the competition. But while it is true that this variance is unique, the high-WBS series does have two things in common with the other series: all show at least a brief downward dip in the middle of the contest, after puzzle 4 or puzzle 6, and then all climb upward from that dip.

Figures 5 through 8, which give the catecholamine responses, must be interpreted with caution. In the E series missing data reduce the number of cases to between two and four.[3] Less consequentially, the number of cases in the NE series is reduced to five. Obviously, estimates become more problematical with smaller numbers of observations.

Figure 5 presents the E series. In the clutter of trend lines, that for the high-WBS group is notable in several respects. It actually begins at a level of zero elevation, much lower than any other, then climbs to the highest of any group for measures 2 and 3. Just before the beginning of the competition there is a great spike, followed by a jerky but typically high trend line to the end of the series. Unfortunately, the spike at measure 4 is based upon only two cases. To mute the effect of this estimate and to provide a sense of the overall E pattern, I have presented a second view of the series. A robust nonlinear data smoother (running medians) developed by Velleman (1980) yields a trend line based upon averaging adjacent values with a particular measurement point. Hence, in this case it downplays a value like that in the spike just prior to the onset of the competition. The results are presented in Figure 6, where the distinctiveness of the high WBS group is again found to be dramatic.

Figure 7 presents the NE patterns. The high WBS group is special, but in a wholly unanticipated way. The initial NE value for the group is the highest of the entire series and the highest to be found anywhere in the figure, a full 129% above the minimum baseline. The series stays high through the first four measurements, again showing a spike, though much smaller, at sample 4. But then the trend line plunges to much lower values through most of the puzzle solving, before once again climbing to high levels at the end. The smoothed version of the data for all three groups is given in Figure 8. Obviously, much is lost in Figure 8, but what remains is a very interesting and distinctive pattern for the high WBS group.

Indeed, the general shape of their response curve is the opposite of that for the others, with the high start, the low middle, and the high finish. What these patterns might mean must be left to endocrinologists to explain. I can only underscore the point that the high WBS group is again found to be very different indeed.

Discussion

This study is only a beginning. Nonetheless, the findings presented here are much more than suggestive. The simple and intriguing fact is that the high serotonin individuals are indeed a special lot. A behavior pattern which had been tied to WBS only through questionnaire data has now been given striking physiological representation. The dynamics of the best understood of the plasma hormones here presented, cortisol and epinephrine, reflect for the high WBS group precisely the kind of internal response that one would expect from aggressive competitors in the context of rivalry. The patterns for ACTH and NE, while fascinating and equally distinctive, are much more mysterious.

There are some complementary studies in humans (Lewis and Sherman, 1984; Ward, Mefford et al., 1983; Ward, Parker et al., 1983) and in subhuman primates (McGuire, Raleigh, and Brammer, in press). Most intriguing is a new line of research by McGuire and his colleagues (personal communication) testing whether pharmacological enhancement of central nervous system serotonergic function raises the probability of becoming dominant. These tests have been run on groups of male vervet monkeys. After the experimenter has removed the dominant male from the group, a randomly chosen male is treated with fluoxetine, a drug which increases serotonergic function. In each of the three tests run, the treated male became dominant! (An early report from the larger project of which this is a part is Raleigh et al., in press.)

With respect to the investigation at hand many fascinating and important questions arise. Findings inevitably lead to questions. But the crucial point here is that there indeed are strong findings. All indications are that whole blood serotonin is a biological property of fundamental significance for behavioral political science.

Notes

I am grateful for the financial support for this research provided by the Harry Frank Guggenheim Foundation, and by the General Clinical Research Program RR59 of the National Institutes of Health. Dr. Barry M. Sherman, Professor of Internal Medicine and Director of the Clinical Research Center at the University of Iowa, provided facilities, medical staff, technical support, supplemental data, and advice. Dr. Michael McGuire, Professor of Psychiatry and Director of the Biobehavioral Science Program at the University of California, Los Angeles, provided technical support and advice. I am most grateful for these invaluable contributions.

1. Serotonin, also known as 5-hydroxytryptamine (or 5-HT), is a hormone with neuroregulatory functions and is found in the central nervous system, in the blood, and in several other body locations. Blood serotonin is relatively stable, and not subject to significant seasonal or daily rhythms, or to short-term perturbations as a function of situation (for example, diet changes or social encounters). How serotonin works its effects, and exactly what those effects are, are matters of limited understanding at this point. Direct tests of its role in the dynamics of human behavior, few in number, are ambiguous in their findings. For extensive and revealing work on animal models, however, see McGuire et al. (1982, in press), McGuire et al. (1983), Raleigh and McGuire (1980), Raleigh et al. (1981), Raleigh, Brammer, and McGuire (1983), Raleign, Brammer et al. (1983), and Raleigh et al. (in press).

2. The blood samples were taken in the following contexts: sample 1 (1600 hours) when the subject arrived at the hospital and had the catheter placed in his arm; sample 2 (1730 hours) after a quiet period and a standard dinner, followed by the move, along with five other subjects, to the experimental room; sample 3 (1750 hours) after 20 minutes of (seated) social conversation; sample 4 (1820 hours) after the rules for the competition had been given and a short paper-and-pencil questionnaire completed; sample 5 (1840 hours) after puzzles one and two had been finished by the group; sample 6 (1900 hours) after

puzzles three and four; sample 7 (1920 hours) after puzzles five and six; sample 8 (1940 hours) after puzzles seven and eight; sample 9 (2000 hours) after puzzles nine and ten; samples 10 and 11 (2020 hours) after a second questionnaire, some general discussion, and payment for participation. Sample 11 was used for the WBS assay.

3. The number of high WBS cases for each estimate in the epinephrine series is, in order: 3, 4, 4, 2, 3, 3, 3, 3, 3, and 3.

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THE CARRYOVER EFFECT IN PRESIDENTIAL ELECTIONS

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The carryover hypothesis asserts that participants' candidate support behavior in the general election is affected by their preferences for their party's nomination. This paper examines data from activist and national sample surveys for evidence of a carryover effect in the 1980 election. There is a detectable carryover effect on voting behavior both for activists and for citizens generally. Among activists, where evidence about candidate support behaviors beyond voting is available, the effect on such activity is substantial, and increases with the effort associated with the general election activity in question.

The American presidential selection process is sequential, with the intraparty nomination campaign followed by the interparty general election. This paper examines the consequences of participation in the first stage for both candidate and party support in the second. The carryover hypothesis asserts that participants' behavior in the general election will be affected by candidate support in the nomination phase. In particular, support of candidates losing the nomination will depress general election activity below the levels expected based upon other predictors such as perceived candidate and party differentials, ideology, party identification, or past levels of activity. Sullivan (1977–1978, p. 637) stated the hypothesis succinctly: "The long pre-convention campaign can only serve to increase the psychological investment each delegate has in his/her candidate. These facts, we think, make it even more difficult for losers to accept the convention outcome and recommit their energies to the winner."

Sullivan's view is in accord with those who see the contemporary nomination process as destructive of the parties' ability to form strong coalitions during the fall campaign (Kirkpatrick, 1976; Polsby and Wildavsky, 1980). In this view, the open character of the process heightens tensions within the parties during the nomination stage, contributing to the fragmented nature of American politics (Epstein, 1978; Wayne, 1984). This relationship between the two stages may contribute to the carryover effect described by Sullivan.

Despite this prevailing understanding of presidential nominations, studies of voting have not incorporated the carryover effect into their explanations of voting choice. Perhaps more surprisingly,

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 80 NO. 1 MARCH 1986

explanations of activist participation have not considered the possibility of a carryover effect. The influence of the Downsian model of voting based upon expected party differentials is substantial (Downs, 1957). In an admirable attempt to explain party activism using the rational choice perspective, John Aldrich (1983, p. 982) explicitly states intraparty dissension is irrelevant to the decision to participate in the general election campaign. The utility-maximizing citizen during the general election, then, is one who forgets the possibly more desirable candidates who might have been nominated and makes the most of the choice he or she is offered. For activists in the context of a general election who are committed to the party and its prevailing ideology, calculating candidate or party differentials without regard for nomination preferences is presumably a relatively easy thing to do.

Data Sources and an Analytic Model

To test for a carryover effect, we require a model capable of measuring the effect of intraparty nomination preferences in the context of the interparty general election. Data from 1980 are available for a sample of Iowa party activists and for the national electorate. Delegates to the two Iowa state presidential nominating conventions were surveyed in June of 1980 before the national conventions, and contacted again immediately after the November election.[1] Similar data are available on the national electorate from National Election Study (NES) 1980 surveys. For the first time, NES conducted a study of the entire presidential campaign by surveying two independent panels at several points during the nomination and general election stages (Markus, 1982). In order to increase the number of cases, I have pooled the first waves of the major and minor samples.[2] These were then matched with the postelection waves to provide information on respondents during both stages of the process.

The model permits us to observe the effects of nomination and general election preferences on participation in the November campaign. Additionally, insofar as possible, it controls for "predispositional" factors which may stimulate (or depress) an individual's activity in the general election. In its simplest form, the model may be expressed as follows:

$$Y = a + b_1N + b_2G + b_3P + e, \qquad (1)$$

where Y = general election participation for presidential candidates or the parties; N = nomination candidate preference; G = general election candidate and party differentials; and P = predispositions to participate.

The dependent variables are constructed to reflect the decision to participate (or remain inactive) as well as the partisan direction of the participation. The coefficient associated with nomination preference estimates the carryover effect from the nomination to the general election stage. Measures of the predisposition to participate are included to control for the possibility that people may be drawn to participate in nomination campaigns who are not in the pool of those the winning nominee (or the party) could realistically hope to recruit for the general election campaign.

Results from the Activist Data

Because the measures available to operationalize equation (1) are richer for the activist data, I begin with a discussion of the Iowa results before moving to an analysis of the NES data.

The model for the Iowa activists is as follows:

$$Y = a + b_1N + b_2G_1 + b_3G_2 + b_4P_1 + b_5P_2 + b_6P_3 + b_7P_4 + e, \qquad (2)$$

Table 1. Nomination, General Election, and Predispositional Predictors of Activists' General Election Participation

Independent Variables	Presidential Vote	Involved in the Presidential Campaign	Presidential Activity Score	Party Activity Score
Carryover Effect				
Nomination Candidate Preference	.015	.148***	.288***	-.055
General Election Comparisons				
Candidate	.421***	.242***	.297***	.089
Party	.182***	.077	-.057	.106
Predispositions				
Ideology	.121*	.220**	.066	.142*
Past Activity	-.006	.045	.094	.187***
Party Offices	.076*	.076	.094	.066
Party Identification	.156**	.087	-.034	.356***
Adjusted R^2	.804	.654	.444	.696
N	589	599	599	573

Note: Cell entries are standardized regression coefficients.

*Significant at .05 level.
**Significant at .01 level.
***Significant at .001 level.

Source: Author's surveys.

where

Y = general election participation, including voting, getting involved in the presidential campaign, and engaging in any of several intensive activities on behalf of the party's nominee. I also include a measure of intensive activity for the party. All are coded to reflect whether the respondent participated, and the partisan direction of the participation (second wave).[3]

N = prenomination candidate preference (first wave).

G_1 = general election candidate preference composed of relative ideological proximity to the Democratic and Republican candidates, and the relative evaluation of the two candidates (second wave).

G_2 = general election party preference composed of relative ideological proximity to the parties, and evaluation of the parties (second wave).

P_1 = self-placement on the liberal–conservative continuum (first wave).

P_2 = level of activity in past campaigns (first wave).

P_3 = number of official party offices held by the activist (first wave).

P_4 = strength and direction of party identification (first wave).

I have noted whether the variables were measured in the prenomination wave of the survey (first wave) or after the general election (second wave).[4]

Table 1 exhibits the analysis of three indicators of participation in the presidential campaign, plus a measure of party activity. I present the presidential support measures in rough order of the effort they required of the activist. Voting is the least demanding form of participation. The second is a response to a general question in the postelection survey asking whether the respondent was "involved" in any way in the fall presidential campaign. The activity scores for the presidential nominee and for the party are composed of identical items asking about relatively demanding forms of participation, including clerical support work, canvassing, and other organizational work.

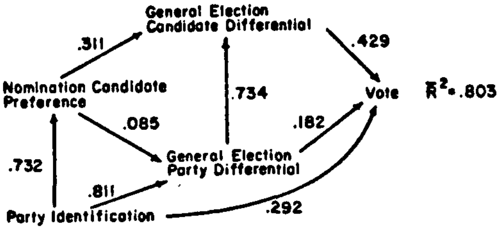
The carryover hypothesis receives mixed support in Table 1. Activists' nomination preferences had no independent effect on voting participation. Indeed, activists were most affected in their voting by their comparisons of the two parties' nominees. Of somewhat less importance were comparisons of the two parties during the general election period, party identification, and ideology. Supporters of nomination losers were apparently not dissuaded from voting for their party's nominee.

The same cannot be said, however, for more active involvement in support of the first round winners. Nomination candidate preference had a significant effect on both the general involvement indicator and on organizational activity for the presidential nominees. Indeed, the results suggest that the more demanding the form of support for the presidential candidate, the greater the carryover effect of nomination preferences. The psychological investment demanded by the first, intraparty stage of the process may be easier to overcome in the case of a relatively "easy" act like voting. But becoming more actively involved, especially participating in the candidate's organization or campaigning face-to-face for a candidate who was the opposition in the first round, may be psychologically very difficult.[5] The relative effect of the general election comparison between the two major party nominees accordingly becomes weaker—and the prenomination commitment to a losing candidate more important—in determining whether an activist becomes involved. Thus, the expectation of a significant carryover effect from the first stage holds up for participation beyond voting.

Interestingly, there is no evidence that party activity of exactly the same sort as measured by the presidential activity score is affected by nomination preferences. Neither do the general election candidate or party comparisons affect activists' willingness to work for their party. Rather, the predispositional variables, chiefly partisan identification and past levels of activity, explain participation for the parties.

The analysis in Table 1 may underestimate the carryover effect, since candidate preference during the nomination stage may affect general election candidate and party comparisons, which in turn have an impact on participation in the second stage. The path analysis of vote choice presented in Figure 1 provides a way of testing for the total effect of preference for the nomination candidate as it worked indirectly through general election candidate and party comparisons. The model shows that nomination preference had a weak (but significant) effect on the general election comparison between the parties, and a stronger impact on the comparison between Carter and Reagan. Supporting the loser during the nomination campaign influenced activists to be less favorable toward the party's nominee in the personal and ideological compari-

Figure 1. Path Analysis of Activists' General Election Voting


General Election Candidate Differential
.311
.429
Nomination Candidate Preference
.734
Vote
$\bar{R}^2 = .803$
.085
.182
.732
General Election Party Differential
.811
Party Identification
.292

Source: Author's surveys.

Table 2. Compound Path Analysis of Activists' General Election Presidential Support

	Presidential Vote			Involved in the Presidential Campaign			Presidential Activity Score		
Independent Variables	Direct Effect	Indirect Effect	Total	Direct Effect	Indirect Effect	Total	Direct Effect	Indirect Effect	Total
Nomination Candidate Preference	—	.175	.175	.197	.096	.293	.317	.109	.426
General Election Candidate Differential	.429	—	.429	.256	—	.256	.293	—	.293
General Election Party Differential	.182	.315	.497	—	.187	.187	—	.214	.214

Note: The estimated path model is presented in Figure 1.

Source: Author's surveys.

sons with his opponent during the second stage.

In Table 2 I report a compound path analysis of the effects of nomination preference and general election candidate and party comparisons on the three measures of general election participation for the presidential candidates. The models estimated for involvement beyond voting and organizational activity for the candidate are identical to that shown for voting. The carryover effect exceeds the total effects of either the general election candidate or party comparisons for both measures of activity beyond voting, when we take into account the indirect effect of nomination preference. Of course, the general election candidate and party comparisons together remain stronger in their effects on campaign activity than the carryover effect from the nomination stage.

In sum, the analysis in Figure 1 and Table 2 demonstrates that nomination preference affected general election participation from voting through more active forms of involvement by affecting activists' general election calculus. The carryover link between nomination preferences and general election candidate support is upheld, whereas a simple expectation that participants will base their general election activity upon comparisons of candidate and party differentials requires modification. The activists in our sample come closest to a model of choice based only on general election alternatives in their voting behavior; but as the costs of a mode of participation in the second stage go up, the commitment activists made during the first stage affects to an increasing extent their willingness to participate for the nominee. These findings suggest that rational choice models such as Aldrich's (1983) should find a way to incorporate intraparty conflict if they are to comprehend party activism in the general election.

A Partial Replication with National Election Study Data

The model employed to estimate the carryover effect from the National Election Study data is similar to equation (2), but the selection of variables is more impoverished. We have but two dependent measures: the vote and a general indicator of campaign involvement.[6] The independent variables include nomination preference and a general election candidate comparison as the basis for comparing the effects of the nomination with the general election stage. The "predispositional" variables include ideology, how often the

Table 3. Nomination, General Election, and Predispositional Predictors of General Election Participation

Independent Variables	Presidential Vote	Involved in Campaign Beyond Voting
Carryover Effect		
Nomination Candidate Preference	.043	.034
General Election		
Candidate Differential	.648***	.516***
Predispositions		
Ideology	.053	.069
Past Voting Activity	.107*	.129*
Education	.109*	.192**
Political Interest	.030	.124*
Party Identification	.129*	.105
Adjusted R^2	.621	.410
N	774	772

Note: Cell entries are standardized regression coefficients.

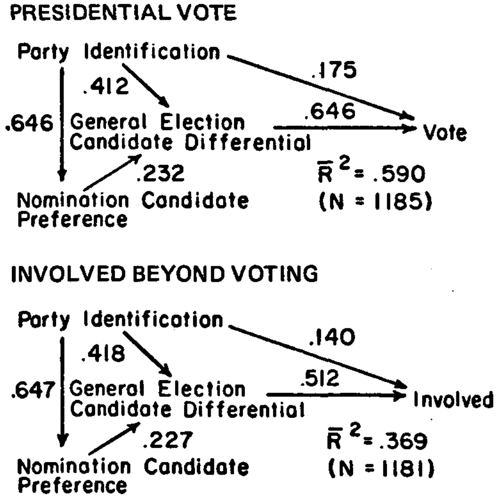
*Significant at .05 level.
**Significant at .01 level.
***Significant at .001 level.

Source: National Election Study 1980 survey.

respondent reported voting in past elections, education, interest in politics, and party identification.[7]

Table 3 presents the results from the full model, and Figure 2 depicts the path analysis from a simplified version designed to test for an indirect effect of nomination preference on general election participation. The findings with respect to voting are very similar to those reported from the Iowa activist surveys. There is no evidence of a direct effect of nomination preference on November voting in Table 3, but Figure 2 shows that first-round candidate preference had a modest impact on the comparisons citizens made between general election nominees. Thus, a compound path analysis reveals a weak total effect of nomination preferences on voting in the general election of .150. The findings pertaining to involvement most closely parallel those for general party activity among Iowa activists. Table 3 shows no evidence of a direct carryover effect, although again an indirect effect of nomination preference is present in Figure 2, resulting in a weak total effect of .116.

Figure 2. Path Analysis of General Election Participation


PRESIDENTIAL VOTE
Party Identification
.175
.412
.646
General Election Candidate Differential
.646
Vote
.232
$\bar{R}^2$ = .590
Nomination Candidate Preference
(N = 1185)
INVOLVED BEYOND VOTING
Party Identification
.140
.418
.647
General Election Candidate Differential
.512
Involved
.227
$\bar{R}^2$ = .369
Nomination Candidate Preference
(N = 1181)

Source: National Election Study 1980 survey.

Conclusion

That there is evidence of a carryover effect, however slight, on mass participation in the general election is consistent with the expectation that contemporary nomination campaigns intrude on the interparty fall campaign, even for the ordinary citizen. Among more partisan and committed activists, there is clear evidence of a stronger carryover effect, especially for presidential campaign activities of the sort activists uniquely contribute. It is this sort of support candidates can ill afford to lose, since the secondary and tertiary effects may translate into a significant number of lost votes. In years when the opposition has no nomination fight, these losses could spell defeat. That the carryover effect does not seem to contaminate activity for the party ticket generally should help calm the fears of those who see presidential nomination races as sapping the vitality of the party organizations.

Appendix

The response rate to the Iowa activist survey was about 58% to the prenomination wave, and 57% to the postelection wave. Comparisons between the first and second waves show that nonresponse to the postelection survey created no serious differences (details are available from the author on request).

The questions used to estimate equation (2) are presented below:

Presidential vote (second wave): "How did you vote in the 1980 presidential election?" Response categories were "Reagan," "Carter," "Anderson," "Other," and "Didn't vote."

Involvement in the presidential campaign (second wave): "Please indicate which of the following general election campaigns you were actively involved in during the fall of 1980 (check as many as apply)." Responses analyzed in this paper included those who checked "Democratic Candidate" or "Republican Candidate" opposite "Presidential Campaign."

Presidential and party activity scores (second wave): "Which of the following activities, if any, did you perform on behalf of the campaigns listed below, between the summer conventions and the November election?" Activities respondents could check were "clerical work," "door-to-door canvassing," "telephone canvassing," "arranging coffees, socials," "fundraising," "writing ads, press releases," and "planning strategy." For each activity, respondents could check boxes under the headings (for Democrats) "For Carter" and "For the Ticket," or (for Republicans) "For Reagan" and "For the Ticket."

Nomination candidate preference (first wave): "Please rank your preferences for President among the following candidates from (1) most favored, to (6) least favored." If a Democrat ranked Carter higher than Kennedy, he or she was scored as favoring Carter, and so forth.

G_1, G_2 (first wave): Relative ideological proximity to the two parties' nominees was constructed from an ideological self-placement question ("How would you describe your own political philosophy?" Respondents were given a five-point scale ranging from "Very Liberal" through "Very Conservative") and a perception question ("How would you describe the political philosophy of each of the following individuals and groups?" The same five-point response scale was used for this item, and respondents placed several leading political figures and the political parties). Respondents rated political leaders and the parties in response to the following question: "Please indicate your opinion of each of the following individuals and groups." Responses ranged on a five-point scale from "Very Favorable" through "Very Unfavorable." The appropriate ideological proximity and ideological proximity scores were combined in additive indexes to create G_1 and G_2.

P_1 (first wave): "How would you describe your own political philosophy?" Answers ranged on the same five-point scale described above.

P_2 (first wave): "Which of the following activities, if any, have you performed in political campaigns?" An additive index was created out of the activities identical to those used to build the presidential activity score from the second wave (see above).

P_3 (first wave): "Please indicate which, if any, of the following positions you now hold or have held in the past." Response categories included local party committee (member or chair), other local party office, congressional district party committee, and state central committee membership.

P_4 (first wave): "How would you describe your own party affiliation?" Responses ranged on a seven-point scale from "Strong Democrat" through "Strong Republican."

Notes

An earlier version of this paper was presented at the 1983 Annual Meeting of the American Political Science Association. I am grateful to Charles N. Brasher for his outstanding work as a research assistant. Generous grants from Grinnell College supported the activist surveys, and the Inter-University Consortium for Political and Social Research provided data on the national electorate (Study Number 7763). None of the above is responsible for any errors of analysis or interpretation.

1. The details of the Iowa survey are reported in the Appendix.

2. Respondents from the first "P" wave were combined with those from the first wave of the "C" panel to form a pooled preconvention sample. The post-election waves from both panels were likewise pooled to match the nomination samples.

3. Nonparticipation is coded as a zero; a negative score is support of Reagan (or the Republican Party); a positive score reflects support of Carter (or the Democratic Party). In the case of voting, a zero means either nonvoting or a vote for John Anderson.

4. The independent variables are coded so that a negative score is a pro-Republican (or pro-Reagan) score; a positive score is pro-Democratic (or pro-Carter); and a zero indicates indifference or absence of the variable in question. For prenomination candidate preference, a zero indicates the respondent preferred a candidate other than the party's nominee, a -1 is a nomination preference for Reagan, and a 1 is a preference for Carter.

5. I assume that becoming active in a general election campaign depends wholly upon the individual's willingness to participate. This is reasonable, since campaign organizations are open and permeable, and because the nominee has every incentive to include as many participants as are potentially available.

6. The NES measures of campaign involvement beyond voting do not identify the candidate on whose behalf the individual was active. I assume the activities were in support of the party for which the respondent voted at the presidential level, but we cannot be certain the individual was active in the presidential campaign. Activities were talking with others to influence their vote, attending meetings or rallies, working for a party or candidate, and belonging to a political club. As with the Iowa data, the variables are coded to reflect partisan direction of activity (negative is pro-Reagan or pro-Republican, positive is pro-Democratic or pro-Carter), and a zero indicates no activity (or a vote for Anderson).

7. I employ candidate preference rather than primary vote as the measure of nomination preference. Among those voting in a presidential primary, the correlation between the first choice variable and reported primary vote is substantial ($r = .81$). Using the primary vote, however, does reduce the N substantially. The general election candidate differential is a comparison of the candidates' scores on the thermometer questions. Because the nomination is a partisan process, and to promote equivalence with the activist data, I analyze only party identifiers (including independent leaners).

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PRESIDENTIAL ELECTION COALITIONS IN 1984

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This report provides an update of major characteristics of the Democratic and Republican party coalitions for the recent U.S. presidential election. Earlier reports published in this Review *have provided this kind of analysis from 1952 to 1980.*

The 1984 presidential election presents the occasion to conduct an analysis of electoral coalitions again, as has been done for these elections beginning in 1952 (Axelrod, 1970, 1974, 1978, 1982). As before, the analysis requires an iterative procedure to correct for the overreporting of the turnout and the vote for the winner.[1]

The coalitions are described in terms of the three components of a group's contribution to a party; namely, the size of the group, its turnout, and its loyalty to the given party. The contribution of a group to the electoral coalition of a party is the proportion of all the party's votes that come from members of that group. The loyalty of a group to a given party is simply the proportion of the votes of that group which are cast for that party.

The 1984 election was a landslide. The Democrats received only 40.5% of the popular vote, their second lowest percentage since 1952. The Republicans received 58.8%, up just over 8% from their bare absolute majority in the three-way race of 1980. The turnout for the 1984 election was a mere 53% of those over 18 years old, consistent with the poor turnouts of the last dozen years (Table 1, column 25).

What happened to the "traditional" coalitions of the two major parties? Let us look first at the Democrats (Table 1). The Democrats are often thought of as a coalition of diverse overlapping minorities: the poor,[2] blacks (and other nonwhites), union families, Catholics and Jews, Southerners, and city dwellers. As in 1980, the Democrats did well with three of these groups: the poor, blacks, and city dwellers (cols. 19, 20, and 24). These three groups were each at least 25% more loyal to the Democrats than was the country as a whole (cols. 27, 28, and 32). But these three groups were relatively small, each containing no more than 12% of the population (cols. 7, 8, and 12). Moreover, the poor and the blacks had low turnout (cols. 13 and 14). Once again, as in 1980, the Democrats did not do nearly as well with the larger groups in their traditional coalition: the union families, the non-Protestants, and the Southerners (cols. 21-23). These groups were only 3% to 14% more supportive of the Democrats

Table 1. The Democratic Coalition, 1952–1984

	Percentage Contribution =						(Size ×						Turnout ×						Loyalty) ÷						(NT×NL)	
Year	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	NT	NL
1952	28	7	38	41	20	21	36	10	27	26	28	16	46	23	66	76	35	68	47	83	59	57	55	51	63	44.4
1956	19	5	36	38	23	19	25	9	26	25	29	14	40	23	64	72	39	63	47	68	55	53	52	55	60	42.0
1960	16	7	31	47	27	19	23	10	25	25	34	13	46	31	60	74	50	74	48	72	66	82	52	65	64	49.7
1964	15	12	32	36	21	15	19	11	23	26	28	12	45	42	69	72	49	65	69	99	80	75	58	74	63	61.1
1968	12	19	28	40	24	14	16	11	24	26	31	10	44	51	61	68	53	63	44	92	51	61	39	58	62	42.7
1972	10	22	32	43	25	14	12	11	25	31	34	8	37	47	58	65	44	60	45	86	45	45	36	61	56	37.5
1976	7	16	33	35	29	11	9	11	23	30	32	8	32	44	62	55	45	58	67	88	63	57	53	61	54	50.1
1980	5	22	31	31	39	12	5	12	25	31	34	8	34	45	57	54	50	51	71	88	50	44	47	69	54	41.0
1984	10	25	30	39	32	7	9	12	20	33	32	8	28	41	59	59	45	58	66	81	55	47	44	70	53	40.5
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

Year	Percentage Deviation in Loyalty to Democrats					
1952	+2	+38	+14	+12	+10	+6
1956	+5	+26	+13	+11	+10	+13
1960	−2	+22	+16	+32	+2	+15
1964	+8	+38	+19	+14	−3	+13
1968	+1	+49	+8	+18	−4	+15
1972	+8	+49	+8	+8	−2	+24
1976	+17	+38	+13	+7	+3	+10
1980	+30	+47	+9	+3	+6	+28
1984	+25	+40	+14	+6	+3	+29
Column	27	28	29	30	31	32

P	Poor (income under $3,000/yr. before 1980; $5,000/yr. since 1980)
B	Black (and other nonwhite)
U	Union member (or union member in family)
C	Catholic (and other non-Protestant)
S	South (including border states)
CC	Central cities (of 12 largest metropolitan areas)
NT	National turnout
NL	National loyalty to Democrats

Table 2. The Republican Coalition, 1952–1984

Year	Percentage Contribution =						(Size ×						Turnout ×						Loyalty) ÷						(NT×NL)	
	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NT	NL
1952	75	99	79	75	87	84	64	90	73	74	72	84	72	67	61	58	73	61	56	57	61	61	57	57	63	55.1
1956	84	98	78	75	84	89	75	91	74	75	71	86	67	64	58	56	69	60	59	59	63	62	60	60	60	57.4
1960	83	97	84	90	75	90	77	90	75	75	66	87	70	68	65	61	71	63	50	51	55	63	50	52	64	49.5
1964	89	100	87	80	76	91	81	89	77	74	72	88	67	66	61	60	68	63	40	42	45	44	38	40	63	38.5
1968	90	99	81	80	80	92	84	89	76	74	69	90	65	63	62	60	66	62	44	47	46	49	47	45	62	43.4
1972	93	98	77	70	73	95	88	89	75	69	66	92	58	57	55	53	62	55	61	66	63	65	60	63	56	60.7
1976	97	99	80	76	74	98	91	89	77	70	68	92	56	56	52	54	59	54	49	52	52	53	49	49	54	48.0
1980	98	90	80	75	66	96	95	88	75	69	65	92	55	55	53	54	56	54	52	56	54	54	51	53	54	50.7
1984	97	96	83	69	72	92	91	88	80	67	68	92	56	55	52	51	57	53	61	65	63	62	60	61	53	58.8
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

NP Nonpoor (income over $3,000/yr. before 1980; $5,000/yr. since 1980)
W White
NU Nonunion
P Protestant
N Northern (excluding border states)
NCC Not in central cities of 12 largest metropolitan areas
NT National turnout
NL National loyalty to Republicans

Year	Percentage Deviation in Loyalty to Republicans					
1952	+1	+2	+6	+6	+6	+2
1956	+1	+1	+5	+4	+2	+2
1960	0	+1	+5	+13	0	+2
1964	+1	+3	+6	+5	−1	+1
1968	0	+3	+2	+5	+3	+1
1972	+1	+5	+3	+4	−1	+2
1976	+1	+5	+4	+5	+1	+1
1980	+1	+5	+4	+3	+1	+2
1984	+2	+7	+4	+3	+1	+2
Column	27	28	29	30	31	32

than was the nation as a whole (cols. 29-31).[3] The Democrats did a little better with union families and non-Protestants after their abysmal performance in 1980 (cols. 21-22) but Mondale's attractiveness to the poor, blacks, and Southerners was not quite as strong as Carter's (cols. 19, 20, and 23).

The Republicans in 1984 once again attracted a majority of all their traditional groups: the nonpoor, whites, nonunion families, Protestants, Northerners, and those outside the central cities (Table 2, cols. 19-24). In fact, the Republicans got at least 60% of each of these traditional groups. Moreover, their effectiveness was magnified by their very large size (cols. 7-12).

In 1984, as in 1980, there was a noticeable difference in how women and men voted. This time women voted 44% Democratic, while men voted only 36% Democratic. Since women were 56% of the sample and had the same turnout as men, they provided 61% of the Democratic votes.[4]

The basic structure of party coalitions is weakened but still recognizable: both major parties still attract a disproportionate share of all six of their traditional groups. The Republicans have now won four of the last five presidential elections, and are finally beginning to close the sizable gap in party identification that the Democrats have always enjoyed. The number identifying themselves as Democrats is still 37% (down 4% from 1980), compared to 28% who identify themselves as Republicans (up 4%).[5] Since the Republicans attracted 59% of those under 30 years of age, this trend might well continue. In short, the advantages for the Republicans and the problems of the Democrats are deeper than the personalities of their respective candidates in 1984, and are reflections of basic trends cutting across the traditional cleavages in the American population.

Notes

I thank Louis Erste, my research assistant. I also thank the Institute of Public Policy Studies of the University of Michigan for financial support, and the Inter-University Consortium for Political and Social Research for making the survey data available.

[1] The survey material is again from the Survey Research Center of the University of Michigan; this time the total sample size was 5,753, and the post-election sample size was 2,247. All definitions and procedures used here are as described in the original article, except that for greater accuracy the 1980 and 1984 contributions have been calculated directly from the adjusted vote rather than from the formula at the top of Table 1. Several minor corrections have also been made in earlier figures.

[2] The definition of "poor" is a family whose income is under $5,000 per year since 1980, or $3,000 per year before then. The categories of the survey do not allow a finer gradation of income to reflect inflation. The amount of inflation is indicated by the Consumer Price Index, with 1967 = 100. The Index was 125 in 1972, 170 in 1976, 247 in 1980, and 316 in 1984.

[3] This survey data apparently exaggerates the Democratic vote in the South, including the border states. The election returns show only 38% of the vote there went Democratic which is even less than the national average. I thank Paul Abramson for pointing this out.

[4] Census figures indicate that women were actually 53% of the voting age population. If so, their contribution to Democrats would have been 58%.

[5] There are also 30% independents (up 5%), and 5% with no preference (down 6%).

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REVIEW ESSAY

Antonio Gramsci: Cronache torinesi (1913–1917).
Antonio Gramsci: La città futura (1917–1918).
Antonio Gramsci: Il nostro Marx (1918–1919). Edited by Sergio Caprioglio. (Turin, Italy: Guilio Einaudi editore, 1980, 1982, 1984. Pp. xii + 898; 1033; and 707. L. 35,000; L. 35,000; L. 40,000.)

The Two Revolutions: Antonio Gramsci and the Dilemmas of Western Marxism. By Carl Boggs. (Boston: South End Press, 1984. Pp. xii + 311. $9.50, paper.)

The Gramscian Challenge: Coercion and Consent in Marxist Political Theory. By John Hoffman. (Oxford: Basil Blackwell, 1984. Pp. xii + 230. $27.95.)

Le Strategie del potere in Gramsci: Tra fascismo e socialismo in un solo paese, 1923–1926. By Leonardo Paggi. (Rome: Editori Riuniti, 1984. Pp. xxxv + 504. L. 30,000.)

Antonio Gramsci, Selections from the Cultural Writings. Edited by David Forgacs and Geoffrey Nowell-Smith. Translated by William Boelhower. (London: Lawrence and Wishart, 1985. £15.00.)

Lingua, intellettuali, egemonia in Gramsci. By Franco Lo Piparo. (Rome-Bari: Laterza, 1979. Pp. xviii + 292. L. 11,000, paper.)

As a political thinker and leader, Antonio Gramsci (1891–1937) defied imprisonment in any intellectual system. Instead, he produced his own theory of politics. His aversion to established intellectual schemes helped him develop the qualities of political theory at its best, including openness to new developments, disinterestedness, the ability to distinguish the essential from the accidental, a determination to speak truth to power, and the discernment of the difference between a paradigm and a utopia.

Gramsci called himself a "critical communist." Today, the idea that one can be critical and a communist at the same time seems outrageous to the powerful in Washington and in the Kremlin. Nonetheless, a critical communist is precisely what Antonio Gramsci claimed to be.

The subject of hundreds of books and thousands of articles, Gramsci has been called everything from a democrat to a Stalinist. However, prospects for understanding Gramsci on his own terms have improved significantly with the recent publication of the first three volumes of his early writings. These volumes, edited by Sergio Caprioglio, constitute the first major breakthrough in Gramscian studies since Valentino Gerratana's critical edition of the *Prison Notebooks* (*Quaderni del carcere*) in 1976.

The scholarly world owes Caprioglio an immense debt. He has convincingly established scores of new attributions, filled in most of the articles partially or totally deleted by Italy's wartime censorship office, cast out some spurious articles, and provided copious annotations. These three volumes—*Cronache torinese, La città futura,* and *Il nostro Marx*—covering the period from February, 1913 to May 1, 1919, deserve prompt translation. Caprioglio plans to produce five more volumes of Gramsci's pre-prison writings.

Caprioglio breaks new ground because his first three volumes demolish the widely held assumption that Gramsci wrote nothing of consequence before the factory council movement. The outlines of the arguments Gramsci advanced in the *Prison Notebooks* are visible in the early articles. The rejection of deterministic Marxism, ridicule of intellectual reductionism, advocacy of non-authoritarian leadership styles, condemnation of "state-worship," contempt for fascist "gladiatorial politics," insistence on the "educative" role of the party, commitment to internationalism, opposition to the working class's acceptance of capitalist "philanthropy," insistence on linking party policy with effective social forces, disgust with "mandarin" bureaucrats in the party and the unions—these and other themes prominent in the "mature" Gramsci's *Prison Notebooks* are anticipated in the "young" Gramsci's journalism. Thus, the thesis that Gramsci's thought shows sharp "breaks," moving from an idealistic and utopian "youth" (1914-1919), to an experiment with the factory councils (1919-1920), to an acceptance of the "Bolshevik" idea of the communist party (1921-1926), and ultimately to a self-consciously theoretical view of politics (1926-1937) is unlikely to survive close inspection of Gramsci's early journalism.

The most prominent quality of Gramsci's mind and spirit was empathy for those at the "margins" of social life. Gramsci was the first important Marxist to look at the political world from the perspective of the "backward" periphery instead of from the "advanced" center. Gramsci combined empathy for the *emarginati* (those kept beyond the margins of mainstream society) with Marx's concept of the class struggle.

Many of the articles and reviews in Caprioglio's three volumes discuss specific injustices committed by those with power and prestige against "the other class," or those beyond the periphery. Accounts of shepherds and peasants who could not afford to buy shoes because protective tariffs from the mainland kept the price of leather artificially high show vividly the economic oppression of Gramsci's native Sardinia. Instead of presenting impersonal statistics, Gramsci evoked the image of innumerable Sardinian men, women, and children having to walk barefoot—summer and winter—on the dried-up riverbeds that substituted for roads. In other articles Gramsci took the side of three provincial women, seeking work in Turin, falsely arrested for prostitution; of an artisan who had been dismissed for the "crime" of arguing with his private employer who had accused the socialist party of being in the pay of the Germans; of some ex-prisoners of war who returned to Turin to find themselves vilified as "cowards"; and of a poor worker killed by a policeman after a nonviolent demonstration against Italy's participation in World War I.

Despite the paucity of references to Benedetto Croce in Gramsci's early writings, critics have claimed that Croce influenced Gramsci's anti-economistic version of Marxism. More likely, Gramsci rejected the positivistic and materialistic Marxism of the Second International because he was repelled by its coldness and impersonality and its tendency to create a caste of scientific "experts" who interpreted Marx and Engels literally. Gramsci's early articles leave little doubt that the roots of his political theory lay in his early personal experiences. Philosophical idealism appealed to him during his university years because it made room for empathy and the subjective element. However, Gramsci did not become a philosophical idealist; he used philosophical idealism to explore his personal experience.

The same can be said for Marxism. Gramsci used Marx's insights to develop a "critical communism." He rejected completely any notion that Marxism comprised an exact "science" of "iron laws."

> There can be nothing in common between critical communism and positivism [Gramsci wrote in October, 1918]. That Marx introduced positivistic elements into his work comes as no surprise. Marx was not a professional philosopher and sometimes [like Homer] he nodded. What is certain is that the essential part of his doctrine has its basis in philosophical idealism. . . . One need only think of the frequent use socialists make of the word 'consciousness,' as in 'class consciousness' and 'proletarian and socialist consciousness'; the conception that one 'is' when one 'knows oneself,' or is conscious of one's own being, is implicit in this language: a worker 'is' a member of the proletariat when he 'knows' himself to be such and works and thinks according to this 'knowledge.' (*Il nostro Marx*, pp. 348-349)

To Gramsci, "Marxism" is not Holy Writ. Marx was neither a messiah nor a shepherd of mindless sheep. As a social analyst, Gramsci wanted the working class to decide how to use his methods of research in responding to changing situations. Marxism was not a code to be enforced from the outside by experts. Despite occasional "incrustations" of positivism, making him sound like a proponent of iron economic laws, Gramsci contended that Marx was a fighter with a simple message: "Proletarians Unite!"

If Carl Boggs had better understood Gramsci's aversion to system-building, he would have spared his readers references to the allegedly "Hegelian-Crocean predisposition" of Gramsci's "early period," to the "Leninist orthodoxy" of the 1924-26 years, and to Gramsci's effort to "transcend the extreme polarities of Luxemburgian spontaneism and Leninist Jacobinism," in the prison writings. We are told that ultimately Gramsci "outgrew Lenin." However, according to Boggs, Gramsci's final teaching incongruously mixed Marx, Machiavelli, Croce, Labriola, Sorel, and Lenin. Nonetheless, although "fragmentary and incomplete," Gramsci left behind "a reconstituted vision of revolutionary change that at once transcended the outlook of the classical Marxists, the spontaneists, and the Jacobin vanguardists," Boggs concludes.

Even in these brief quotations, Boggs reveals his passion for putting Gramsci in boxes. Indeed, what troubles him most is Gramsci's refusal to remain in any of them. Boggs's picture of Gramsci shows him lurching one way and then another, as if trying to touch all bases at once. We are told that Gramsci left a "conflicted legacy," that he could never quite make up his mind whether he favored force or consent, centralization or decentralization, participation by the masses or elite guidance by the party ("vanguardism"), and so on down Boggs's line of jawbreaking antinomies. Then there is the parade of "influences" of early, middle, or late Lenin, of Sorel, of the Machiavelli of the *Prince* (no mention of the *Discourses*), and on and on. Every page illuminates a new "influence."

One may wonder whether other thinkers fit into Boggs's boxes, and why he puts them there in the first place. The answer may lie in Boggs's esteem for system-building. Boggs reports that "bold but not very systematic efforts to break through the barriers of [Marxist] orthodoxy were made by [Antonio] Labriola, Gustav Landauer, Luxemburg, and, on the fringes of Marxism, by Sorel and the Council Communists . . . to seriously address the issue of consciousness."

According to Boggs, in Gramsci's early writings one "searches in vain for any treatment of the state, political power, [or] of the role of the party (not to mention unions and councils. . ." [p. 60]). Apparently he has not searched far enough, for on p. 30, footnote four, he declares that "the bulk" of Gramsci's writings for the period 1916 to 1919 "is contained in Paolo Spriano, ed., *Scritti politici*, published by Riuniti in 1967." Why did Boggs rely on Spriano's 1967 anthology when he could have used the first two of Caprioglio's volumes (published in 1980 and 1982, respectively) plus other published sources to see at once that Spriano's anthology contains only a small fraction of Gramsci's journalistic output for the period described? Caprioglio's research has demonstrated that between January 1916, and May 1, 1919, Antonio Gramsci wrote 853 articles and 188 reviews of plays, covering a total of 2,543 pages of text. Spriano on the other hand reprinted only 57 articles covering 190 pages. If Boggs had examined Gramsci's entire output for the period, he would have discovered in it quite a bit about "the state," "political power," the "role of the party," and maybe even "unions and councils."

In *The Gramscian Challenge* John Hoffman relies exclusively on English translations of Gramsci, thereby placing himself at a disadvantage. Only about one third of Gramsci's writings have been translated into English. According to Hoffman, neither Marx nor Gramsci has properly distinguished and integrated the moments of force and consent into an adequate theory of politics. Marx synthesized force and consent without analyzing either term. Gramsci did the reverse, analyzing force and consent without synthesizing them.

Hoffman's book at times reads like a tract for the converted. The non-Marxist reader may feel like an intruder on a family quarrel. Although in a sense refreshingly polemical, Hoffman at times resorts to incomplete sentences and exclamation points to sustain reader interest. Nonetheless, in criticizing the artificiality of some dichotomies in the literature about Gramsci, Hoffman performs a service.

Hoffman contends that Gramsci identified vulgar materialism with materialism as such, that he had a "mechanistic" and "abstract" view of nature, and that "it is only toward the end of the factory occupations (Spring of 1920) that Gramsci accepts the need for a revolutionary party" (p. 53). I think the first two statements are wrong, and I know the third one is, because the factory occupations occurred in September, 1920, and as early as May 18, 1918, Gramsci wrote that a revolutionary party, "morphologically distinct from any other party," was indispensable to the revolution against capitalism. "The socialist party, wrote Gramsci, does not conquer the state—it replaces it. . ." (*Il nostro Marx*, pp. 29, 30).

The pages devoted to Stalin (pp. 186–98) would have given George Orwell a field day. Hoffman defines Stalinism as "a proletarian state form which combines exceptional violence with exceptional legitimacy" (p. 198). There is something terribly wrong with an approach producing such euphemisms about Stalin, whom Gramsci diagnosed as a practitioner of "statolatry."

Leonardo Paggi's book on Gramsci's political thought and practice from 1923 to 1926 is of major importance. Scrupulously and exhaustively documented, *Le strategie del potere in Gramsci* begins with Gramsci in Moscow, where he served as Italian representative on the Third Communist International's Enlarged Executive. Returning to Italy in May, 1924, Gramsci wrested the leadership of the communist party from Amadeo Bordiga and charted a "new course" aimed at reorganizing the party for closer relationships with ordinary working people. Gramsci opened the party ranks to new elements. Rather than requiring party members to pass a litmus test of loyalty to a doctrinaire version of Marxist-Leninism, Gramsci, elected secretary-general in August, 1924, sought to develop the critical capacities of all party members in various ways. He also sought to bring the Italian party into line with the Comintern's "united front" policy, which Bordiga had fiercely resisted. By "Bolshevizing" the Italian communist party, Gramsci wanted to rescue it from the organizational shambles into which it had fallen under Bordiga. Gramsci never expelled a single individual, and the sinister associations of the word "Bolshevization" should not cloud our appreciation of Gramsci's role as party secretary.

Paggi argues that Gramsci's famed idea of "hegemony" originated in the context of practical political activity. Hegemony was not an academic theory but a strategic doctrine. As Gramsci affirmed in 1924 at Como, "our party must become the party of the greater masses of Italians; it must become the party that realizes the hegemony of the proletariat in the larger framework of alliances between the working class and the peasant masses" (quoted in Paggi, p. 207). If, as Lenin had said, conditions in Russia in 1917 were unique and "unrepeatable," western Europe needed a new strategy for revolution. Paggi documents the fact that, well before he elaborated the idea of hegemony in his *Prison Notebooks,* Antonio Gramsci had worked out its essentials in Vienna in the winter of 1924. As a strategy, revolution must first establish a new moral, intellectual, and cultural leadership to correspond with the increased economic power of the proletariat and the peasantry, and it entailed a politics of alliances with social forces outside the communist party itself. Paggi brings out the originality and subtlety of Gramsci's strategy which differed from Bordiga's in many respects, chiefly that the party could not just sit around and wait for the revolution to happen. Paggi argues convincingly that Gramsci's call in the prison writings for the precedence of a "war of position" over a "war of maneuver" in Italy and western Europe was anticipated in the strategy he had followed as leader of the Italian communist party.

Gramsci scholars will wish to consult Paggi's book, even though it contains major errors of judgment. Paggi does not understand that Gramsci's vision of a new politics directed toward including those groups and social forces hitherto at the margins of society enabled him to transcend traditional dichotomies. Gramsci did not have to argue "consistently" in favor of either Schumpeter's or Rousseau's kind of democracy, or of either Lenin's or Luxemburg's theory of the party. When will even the better Gramscian scholars cease trying to impose their own procrustean categories on Gramsci and realize that Gramsci's thinking was alive and contextual? Gramsci did not fabricate abstract intellectual schemes in advance. He immersed himself in the details of the everyday struggle to check the expansion of Mussolini's dictatorship. He made painful choices about whom he could and could not trust as allies. He did not expound a literal theory, valid for all times and occasions, about the role of the party in the revolution, or about "force," "consent," "power," or any other abstraction. The point is that Antonio Gramsci deliberately broke with the prevailing way of doing political theory in our century. The best means of understanding his new way of theorizing is by attempting as best one can to put oneself in Gramsci's place, by empathizing with his perspective of politics as seen from society's periphery. One is unlikely to understand Gramsci by grading his ideas according to one's own idea of consistency.

Paggi's eagerness to judge Gramsci according to a system-builder's criteria leads him to overlook the basic continuities in Gramsci's life. Thus, Paggi discerns an "idealist" and an "activist" approach in Gramsci's "youthful" writings "in conformity with the dominant currents of Italian and European culture of the first two decades of the twentieth century" (p. 37). Next came a quasi-Machiavellian stage when Gramsci thought of politics as a "science." The Machiavellian Gramsci was attracted to a "Jacobinism" of Sorelian "inspiration." (Both Boggs and Paggi are fascinated by Gramsci's different evaluations of the Jacobin role in the French Revolution.) Such chopping up of Gramsci's life into phases governed by this and that "inspiration" derived from Croce, Lenin, Daniel De Leon, Machiavelli, Sorel, Salvemini, or Mosca makes it impossible to understand what Gramsci himself created. Indeed, why study Gramsci at all if he was only a derivative thinker? In the end, Paggi produces only a more sophisticated version of the tendency we saw in Boggs to erect straw men and to judge Gramsci according to them. To call Machiavelli a "Jacobin" political "scientist," is to ignore all the recent decades of serious scholarship have taught us. The author of the *Prince* also wrote the *Discourses.*

Then there are Paggi's post facto judgments on Gramsci's observations about fascism. In concluding that Gramsci "underestimated fascism as a political force capable of providing the basis for . . . the reorganization of the bourgeoisie" (p. 242), Paggi forgets that Gramsci was supremely conscious that he was not writing a scholarly treatise about events observed from a distance. Gramsci knew that he was both an analyst of and an actor in the struggle against fascism, and one of his objectives was to rally his supporters and keep up morale. His ridicule of the fascists as "monkey people" who thought that they were the best inhabitants in the jungle, but who in fact could only make ridiculously pompous speeches, did not represent his serious political estimate of fascism's capabilities. Gramsci's many articles on fascist violence from 1919 on show how seriously he took the movement that itself was to take his life. He knew the risks he was running in remaining in Italy after Mussolini's speech of January 3, 1925, "resolving" the Matteotti crisis.

If Paggi had only attended more closely to the documentation and resisted his temptation to interpret events in abstract categories, he would have written a better, more readable book. The reader must wade through a thicket of dense prose to find a flower of insight. Even then, he is left at the end of the book feeling uncertain about where Paggi would have him turn to find the real Gramsci, alternately praised as original and exposed as derivative. Thus, the last lines of Paggi's text (followed by an appendix of over 100 pages) declare that while Gramsci succeeded in "enlarging" our notion of politics, he fell short of producing a "critique" of it. Gramsci's thought was burdened by a "perennial internal contradiction" which now must be "resolved" and "surpassed" (p. 381).

Paggi fails to see that precisely through "enlarging" our concept of politics, Gramsci achieved a "critique" of it. From his childhood and adolescence in Sardinia to his prison cell at Turin, Gramsci sought always to fight for a new political order in which "all living men, as many men as possible, all the men in the world will have united in society to work and struggle and better themselves" (letter to his son Delio, quoted in Paggi, p. 380).

David Forgacs and Geoffrey Nowell-Smith have compiled an annotated anthology entitled *Antonio Gramsci: Selections from Cultural Writings,* superbly translated by William Boelhower. The first section contains articles and reviews primarily from Caprioglio's critical edition of Gramsci's early journalism. The remainder of the book includes what the editors call "cultural" topics discussed by Gramsci in the *Prison Notebooks.* Gramsci's notes are grouped under the following headings: "Problems of Criticism," "Pirandello," "Canto X of Dante's Inferno," "Language, Linguistics, and Folklore," "People, Nation, and Culture," "Manzoni," "Father Bresciani's Progeny," "Popular Literature," and "Journalism." The editors provide a useful introduction explaining their criteria for selecting and organizing their texts. They also announce the imminent publication of another Gramscian anthology—on philosophy, science, and religion.

While any effort to make more of the Gramscian corpus available in English can only be welcome, the material is distorted when arranged according to criteria imposed by the editor. Gramsci himself rejected the division of human activity into politics, culture, science, art, etc. "Everything is politics," he declared, including philosophy and culture. By dividing Gramsci's writings into some labeled "politics" and others labeled "culture," the editors obscure precisely what was most radical in Gramsci's architecture of a new politics.

Although the Forgacs and Nowell-Smith anthology of Gramsci's "cultural writings" is welcome, it is no substitute for reading Gramsci's text. The time is overdue for English translations of both Caprioglio's volumes of the early Gramsci and the full text of Gerratana's 1976 critical edition of the *Prison Notebooks*, preferably together with the complete index compiled by Renzo Martinelli and published in 1977 by the Istituto Gramsci in Florence. Then, and only then, can the English-speaking reader who does not know Italian go directly to Gramsci without having him filtered through the lenses of well-intentioned editors.

In *Lingua, intellettuali, egemonia in Gramsci*, Franco Lo Piparo argues that the roots of Gramsci's theory of hegemony extend more deeply into the past than anyone had supposed. Through creatively using Gramsci's elaborate notes of Professor Matteo Bartoli's lectures on linguistics at Turin University in 1912–1913, Lo Piparo demonstrates how greatly Gramsci's linguistic studies influenced his later political thought. Gramsci learned from Bartoli that language is much more than grammar and vocabulary; it is a component of a worldwide process in which language, politics, and culture are linked.

Lo Piparo's methodology contrasts favorably with that of Boggs, Hoffman, and Paggi. Instead of judging Gramsci by ill-defined terms ("Jacobin," "vanguardism," "neo-Hegelianism" and the like) Lo Piparo explores the meaning of key Gramscian terms such as "prestige," "spontaneity," and the "people-nation" as they evolve within the Gramscian texts themselves. In this respect, Lo Piparo's humanistic approach could serve as a model for scholars in the history of political theory.

Lo Piparo helps lay to rest the conventional view of a radical break between the "young" and the "mature" Gramsci. His interpretation reinforces Caprioglio's message. Lo Piparo also shows that the dates for Gramsci's "youth" must be pushed back from 1916 to at least 1912–1913.

Franco Lo Piparo is the first scholar to have investigated thoroughly the impact of linguistic studies at Turin University on Gramsci's political theory. He skilfully relates passages from Gramsci's 1912–1913 lectures notes to passages in the *Prison Notebooks* on the idea of "hegemony."

Generally faithful to what Dilthey called the method of *verstehen*, Lo Piparo lapses into judgmentalism, however, when he dismisses Gramsci's paradigm of the *società regolata* as "fantastic" and as occupying "a marginal place in Gramsci's thought" (pp. 125, 150, n. 71). This statement is surely arbitrary; one might more plausibly argue that the concept of the "regulated society" crowns Gramsci's thought. For the Sardinian revolutionary, the "regulated society" dispenses with both coercive government and conformist social pressure. It is a condition in which human beings really (and not just on paper) "agree to accept the laws spontaneously, freely," rather than through coercion (*Quaderno* 6, Gerratana edition, 1976, p. 764, paragraph 88).

In the study of writers of rank, the interpreter must take pains to place understanding first, and his own criticisms second. Lo Piparo has every right to criticize Gramsci's theory of the regulated society, but as a serious scholar, he should not have dismissed it arbitrarily.

It is in the nature of a paradigm to appear unreachable. Whether Gramsci's idea that men have the capacity to shape the world according to the designs of a new politics committed to including every human being equally is feasible or not, that vision hardly holds a "marginal place" in Gramsci's political thought. The conviction that all the world can be an oasis of freedom and equality and that human beings do not have to continue to live in the desert of the old politics was central to Gramsci's political vision. Antonio Gramsci knew that "a desert is still a desert, even if one sees an oasis with palm trees here and there." He had faith that in time all the world's people will be able to lead the life of self-critical reason and to appropriate the great cultural treasures of the past, hitherto the preserve of restricted elites. Such a happy issue out of our present afflictions would not occur automatically, however. "Pessimism of the intellect, optimism of the will" was his motto to the last.

DANTE GERMINO

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POLITICAL THEORY

Polis and Praxis: Exercises in Contemporary Political Theory. By Fred R. Dallmayr. (Cambridge, Mass. and London, England: The MIT Press, 1985. Pp. xi + 297. $25.00.)

Fred Dallmayr, Packey Dee Professor of Government at the University of Notre Dame, calls this provocative collection of essays "exercises" to signify the exploratory character of his work. The term also indicates the difficulty of Dallmayr's enterprise and reveals his understanding of philosophy as philosophizing. The author's intention is to clarify and redefine salient social and political concepts whose current problematical meanings contribute to the crisis of our time. "Little inquiry," writes Dallmayr, "is required to show the deep opacity shrouding the concept of freedom in contemporary Western thought and its deterioration into an ideological slogan (akin to the status of 'socialism' or fraternity in the East). Mankind . . . is steadiy drifting toward a political cataclysm, on both sides propelled by watchwords or doctrines whose significance is only barely intelligible" (p. 107).

Dallmayr does not attempt to impose a conceptual system of his own. Closed doctrines violate his idea of what philosophy is about. Instead, he conducts a many-sided dialogue of search in the form of reflections on the ideas of leading thinkers who have devoted their lives to formulating a new way of ideas. Dallmayr's philosophic openness is revealed in the range of opinion he canvasses: the thought of Martin Heidegger, Michael Oakeshott, Leo Strauss, Hans Georg Gadamer, Max Weber, Hannah Arendt, Edmund Husserl, Maurice Merleau-Ponty, Juergen Habermas, Michel Foucault, Richard Rorty, Richard Bernstein, Duncan MacRae, and others. The essays restate, compare, analyze, select, and synthesize pregnant ideas that Dallmayr draws from this grand array of thinkers. Each essay is a kind of Socratic conversation.

The book's seven chapters focus, seriatim, on (1) the meaning of political philosophy, (2) "experience" as "praxis," (3) the idea of "power" (4) Heidegger's ontological meaning of "freedom," (5) critical theory and humanism, (6) evaluative discourse in the study of public policy, and (7) politics and communication. Building on foundations laid in *The Twilight of Subjectivity* (1981), Dallmayr reveals the pitfalls of solipsistic freedom; of separating theory from practice and fact from value; of confining experience to sense experience; of understanding power as a term signifying domination and control; of an instrumentalist approach to public policy studies. He leads us toward a post-individualist political philosophy by asking that we consider the fruitfulness of anchoring freedom in "being" (following Heidegger), that we engage in philosophical innovation which maintains continuity with tradition while avoiding slavery to it, and that our political theorizing be action-oriented. Above all, he wishes to persuade us to understand political philosophy as the communication of free minds in a spirit of friendship. He closes his final chapter with a reflection on Oakeshott's thought that " 'the world *sub specie amoris* is unmistakably the world of practical activity. . .'; nevertheless, love and friendship generate a peculiar contemplative engagement, an acceptance of 'whatever it turns out to be' " (p. 223). This is a statement of hope, not of resignation.

Dallmayr's essay on Heidegger's ontology of freedom (which appeared in modified form in *Political Theory* in 1984), is the centerpiece of the book. It presents a fresh and exciting reading of Heidegger and reveals as false, dominant interpretations of his work as fatalistic. Heideggerian freedom is the defining characteristic of the human person, and is grounded in the freedom conceived as "the comprehensive and pervasive dimension of being" as such (p. 124). The problematic of this idea, it seems to me, lies in the difficulty of persuading a modern mind of the plausibility of Heideggerian "being" as a fundamental ontological category.

Polis and Praxis is evocative, erudite, and philosophically rich. Its message will be pondered seriously by persons of good will who are ready to engage, with "rebellious gentleness," in the necessary work of rethinking our political lexicon.

WILLIAM T. BLUHM

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Democracy and the Rule of Law: Liberal Ideals and Marxist Critiques. By Bob Fine. (London: Pluto Press, 1984. Pp. viii + 231. $11.95, paper.)

Bob Fine's analysis of the idea of the rule of law starts in discontent. His disagreement is with two polar perspectives in contemporary Marxist thought on the question of Marxism's relationship to liberalism, particularly the ideology of the latter on the question of the coherence of the liberal conception of the rule of law. The first of these views sees Marxism as compatible with—perhaps even an extension of—liberalism on questions of legality and democracy. In other words, to be faithful to Marxism, socialist thought need not jettison a recognition and appreciation of liberal principles and the idea of the rule of law. Instead of opposing liberal ideals, Marxists can, without sacrificing ideological integrity, incorporate liberal notions of equality and justice within the framework of a Marxist jurisprudence.

But there is a danger in such an accommodation: by incorporating liberal values within a Marxist framework, the liberal forms of class-based authority will be masked and the antinomious character of liberal political theory obfuscated. It is in response to this danger of ideological infiltration that the opposing pole of Marxist thought rejects outright any attempt to accommodate within Marxism any part of liberal political ideology, in particular the idea of the rule of law. Thus, the rule of law is rejected as an obfuscation of liberal forms of power and domination.

Need Marxism be completely antithetical to all facets of liberal thought? Fine thinks not, and he devotes the first two of seven sections in his book to a review of the development of Marx's jurisprudence against a background of the classical jurisprudence of Hobbes, Rousseau, Adam Smith, and Hegel between the seventeenth and nineteenth centuries. In approaching Marx's jurisprudence from the perspective of intellectual history, Fine attempts to demonstrate Marx's debt to classical jurisprudence, and at the same time (in Sections 3 through 6), seeks to show the development of Marx's thought away from the classical view to a point more in keeping with his mature writings on political economy—in particular, the *Grundrisse* and *Capital.*

The last section of Fine's book is devoted to two twentieth century attempts to develop Marx's critique of jurisprudence in a new direction, and in one instance, to displace it. The effort at displacement is that of the late Michel Foucault to transcend the left/right distinctions in favor of an historical/analytical discourse on forms of power. The revisionist attempts at reconstruction are those of the Russian legal theorist Evgeny Pashukanis and the contemporary English Marxist historian E. P. Thompson. Fine takes up Thompson's declaration in the latter's *Whigs & Hunters* (1977) that the rule of law is "an unqualified human good" and finds that in the course of rejecting vulgar Marxist conceptions of law and state (e.g., L. Althusser), Thompson himself "comes close to abandoning Marxist criticism of the rule of law in its entirety in favor of a resuscitated liberalism" (p. 175).

DENNIS M. PATTERSON

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Retrieving Democracy: In Search of Civic Equality. By Philip Green. (Totowa, New Jersey: Rowman and Allanheld Publishers, 1985. Pp. ix + 278. $19.95.)

Philip Green has taken seriously the view that theorists are revolutionaries with a pen. In his book, *Retrieving Democracy,* he attempts to set a revolutionary agenda for the United States. The first step is to dispel the notion that we currently live in a democracy. Although Green finds some admirable qualities about our current system, especially when contrasted with other existing systems, he nevertheless believes it to be a "pseudodemocracy." Control of the polity simply is not in the hands of the demos.

Green's objective is to seek a true democracy, one that captures the spirit of Rousseau's general will without the potential authoritarian qualities that can be attached to it. This is to be achieved through political equality, participation, and the effective use of representation. The excesses of majoritarianism are to be curbed by an expanded use of judicial review.

However, political equality cannot be achieved without social equality. To be civic equals, we must do away with our class system (capitalistic economic divisions) and our caste systems (social divisions based on sex and race). Green focuses upon the division of labor within workplaces as a critical determining factor. Divisions of labor are necessary, but they should be democratically determined. People should be free to determine for themselves the type of work they wish to do.

Green captures his major prescriptions presented within the body of the work in the final chapter. There he poses a program for the future of the United States—a future he believes is closer than we think. His proposals include items such as the social control of corporations, equalization of wages and job opportunities, democratization of trade unions, and the insulation of politics from money.

Retrieving Democracy makes for good reading. The arguments are clear and the writing is concise. Although there is nothing new about the basic contention that to change the political world we must first change the social context in which politics takes place, there is something novel in the way Green blends what is worth keeping in the current system with what must be changed. For example, many of the aspects of liberal politics and the constitutional institutions that are created within advanced capitalistic states become cornerstones of the democratic future envisioned by Green.

How convincing Green is probably is determined more by whether he is preaching to the converted than by how persuasive are his arguments. As with most polemical pieces, documentation is kept to a minimum. Although he has attempted to be practical at the same time he is utopian, I do not know that he has set an agenda for social and political action. I do not know that he has offered the reader much guidance as to what to do about the sexism and racism that could continue to undermine his hopes for a more egalitarian future.

Setting the agenda for radical change in the United States is a serious enterprise. Green is at his best, however, when he is not taking himself so seriously.

RONALD M. MASON

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Sports and Political Ideology. By John M. Hoberman. (Austin, Texas: University of Texas Press, 1984. Pp. 315. $24.50, cloth; $9.95, paper.)

This book offers a fine survey of political theory and its relationship to body fitness. Along the way, Hoberman delves into the different approaches that European intellectuals have taken toward sports. This book is important in that it offers a comprehensive survey of the role of sport ideology in Nazi Germany, the German Democratic Republic, the People's Republic of China, and the U.S.S.R.

Hoberman does not offer any new theories about sports and ideology, but he does provide strong scholarship in his presentation of the views of others. He assumes that every political ideology has an official "anthropology" and that ideologies differ on how they relate to the body. Moreover, he assumes political ideologies have permeated most every form of culture within the various societies and that the conduct of sports in a society reflects the political ideology of the country.

He examines European intellectuals on their views toward labor versus play as human fulfillment. For example Marx and Engels argue for the "primacy of labor" and Ortega y Gasset and Johan Huizinga argue the importance of the play dimension in a society while not negating labor. Next, Hoberman looks at the body as an ideological variable in states. The athletic body has served as a symbol for the body politic and as a symbol for political leaders to manipulate for ideological reasons. According to Hoberman, the fascist view of the athletic body as virile, heroic, masculine, and dynamic contrasts with the Marxists' refusal to play the narcissistic intellectual game. Nazi sport theory emphasized racial heroism while early Soviet sport ideology attacked the competition of bourgeois sports.

Yet as Hoberman observes, modern Soviet views toward sports rejected the earlier attacks on competitive sports when the state realized that there are certain elements of play which the state cannot control. Still, problems exist in Soviet society when the extraordinary Soviet athlete is valued above the masses.

East German political ideology looks at the body as a machine that can be made to perform at levels which are beyond normal expectations by the use of technology and science. Leisure in the East German state cannot be idle, rather it must be productive, and move toward a goal of "all-around human development." Maoist sport in the People's Republic of China on the other hand does not emphasize competition or the development of superior athletes. Sports are good for the masses in that they develop healthy bodies for all. Hoberman then indicates shifts that the post-Mao society has undergone with respect to sports. Sports may be pursued in an effort to develop a productive, technically superior, competitive Chinese athlete who will bring honor to the state.

Although this book is well written and extremely literate, there are some points of disagreement that this reviewer must raise. Hoberman fails to deal with American political ideology and sport, and he should have been able to develop this chapter in the story of sports and political ideology. He decides not to pursue this matter because ideology plays a very modest role in American political life according to Hoberman. I think he erred in leaving out the importance that political ideology has on the conduct of American sports.

There are also other ways to investigate sports and political ideology rather than exclusively centering on what political thinkers think and say. One could just as easily conducted empirical investigations as to the ideological beliefs of various athletes and how these related to their conduct of sports.

Regardless of my minor disagreements on research strategy, Hoberman has written a major book in the field of sports and politics. He makes explicit connection between sports and political ideology in areas which some readers might think are obvious. Yet they were not obvious until Hoberman pointed them out.

JOHN ORMAN

Fairfield University

Anarchism. By David Miller. (London: J. M. Dent and Sons, 1984. Pp. 216. £10.95.)

The tradition of anarchist thought is a rich and varied one which, in the West, represents the most radical challenge to political authority and, more precisely, the modern state. Historians of anarchism James Joll and George Woodcock have provided us with excellent, authoritative accounts of this tradition. Joll's approach is that of the historian of ideas while Woodcock's history is more sensitive to the complex interaction between ideas and sociohistoric contexts. Yet both effectively combine historiography with biography in their respective constructions of "the anarchist tradition." David Miller's recent work on anarchism is designed to complement these two fine histories by focusing on the ideological character as opposed to the historical dynamics of this revolutionary tradition. Anarchism is reassessed as an ideology, and as a system of ideas and set of beliefs about human nature, history, society, politics, economics, the state, revolution, and radical reconstruction. Like other major enduring doctrines of the modern age, anarchism is treated as a coherent world view capable of holding its own when compared with the likes of liberalism, conservatism, socialism, Marxism,and their more contemporary offspring. Thus, this exercise is reminiscent of Daniel Guerin's *Anarchism* with its emphasis upon the relationship between theory and practice. However, unlike Guerin, Miller distances himself from the ethos of the old New Left. His ideological stance, which is revealed early on, is that of a "market socialist" sympathetic to some of the ideas and ideals of the anarchist thinkers while unwilling to endorse anarchism as a theory.

Miller's work is divided into three parts. Part one briefly surveys the "varieties of anarchism"; Part two examines the difficult relationship between anarchist theory and revolutionizing practice; and Part three offers a summarizing evaluation of constructive achievements, critical questions, and noble failures. A preliminary sketch of this radical ideology establishes a common ground shared by all self-proclaimed anarchists and identifies two major premises which hold anarchism together: anti-statism and voluntary association. Yet while there is unity in the anarchist ranks, there is also diversity. Thus, Miller proceeds to analyze three different schools of anarchist thought: philosophical, individualist, and communist. Philosophical anarchism, the intellectual assault on the principle of authority, is repudiated by way of a critique of Godwinian utilitarianism, Stirner's egoism, and the radical neo-Kantianism of Robert Paul Wolff. American anarcho-individualism, the libertarian tradition, is represented by the thought of Josiah Warren, Benjamin Tucker, and Murray Rothbard. Here the discussion centers on individual sovereignty and the radical return to a purified free market system. Anarcho-communism, by far the most substantial anarchist stream in both theory and history, brings us into contact with the discourses of Bakunin, Kropotkin, Malatesta, Berkman, Goldman, and Bookchin on communitarianism, anti-capitalism, mutual aid, positive freedom, and mass revolution. Miller concludes that (1) all the philosophical arguments for rejecting authority are unsatisfactory, and (2) for the most part individualist- and anarcho-communism argue past each other due to their unresolvable views on human nature and radical reconstruction.

These conclusions may be correct, yet Miller's overall exposition and critique of these three variants are simply too brief to do justice to the anarchist perspective. As such, the reader is given only an introductory flavor, a taste of anarchist thought. Perhaps this was his intention. Yet Godwin's *Enquiry Concerning Political Justice* cannot be dissected in less than four pages, even if its enlightenment rationalism betrays certain contradictions, eccentricities, and elitism. Stirner's creed of conscious egoism and nihilistic disobedience is also, perhaps surprisingly, more complex than presented. Further, it can be argued that Stirner is the precursor of a possible fourth school of anarchist thought, existential anarchism. But Miller's most glaring weakness is his failure to include Jeffrey Reiman's reply to Wolff's classic *In Defense of Anarchism* as developed in his *In Defense of Political Philosophy*. The absence of a serious treatment of the Wolff-Reiman debate is disappointing. However, Miller's general portrayal of individualist anarchism and communist anarchism as extremist versions of classical liberalism and communitarian socialism, respectively, is well argued. Both schools are far too one-sided and thus blind to the greater complexities of human identity, socio-political organization, and radical change. Anarchist thinkers are especially weak in economics. Proudhonian mutualism is the most plausible theory that they can offer.

Part two, "Anarchism as a Revolutionary Ideology," the heart of this book, focuses on varying conceptions of human nature and historical progress as they inform anarchist practice, the relationship between anarchist practice and Marxism and syndicalism, organization and strategy, violence and terror, and the New Left. Here the relation between theory and practice is more rigorously analyzed with regard to specific issues. Anarchists emerge from this scrutiny as better theoreticians of human nature than they are of history. While Kropotkin's mutual-aid theory of social evolution comes closest to a theory of history, it, like Marx's historical materialism, is found to be more ideological than scientific. The anarchist-Marxist debate is also well done, with anarchists scoring high points in their critique of Marxism. This attack includes refutations of historical materialism, "scientific socialism," state theory, revolutionary elitism, agency, and post-revolutionary reconstruction. The historic outcome of this debate in turn yielded the "syndicalist synthesis." Miller may criticize the anarcho-syndicalists in theory, but he has a difficult time dismissing them in practice, given the relative success of the Spanish experience. The New Left, unfortunately, is afforded a narrow and stereotypical treatment. Here the focus is on the French "Situationists," and the claim is made that the New Left fundamentally diverted anarchism from its revolutionary ideals because of its emphasis upon creating a parallel, alternative society. Alternativism replaces anti-statist action.

The means-ends uncertainty in anarchist thought draws its arguments from a rich history of noble failures. Miller's judgment is that while anarchists tend to be better means-ends thinkers than Marxists, their strategies have failed while Marxists' have succeeded. Why? Because anarchist organization is almost a contradiction in terms. And, ironically, the most committed anarchists often in the end tend to be radical individualists. Finally, is anarchist terrorism the solution to the problem of means and ends? An examination of the 1890s and 1970s sheds light on this question. While terrorism is ruled out as ineffectual in the long run, so is non-violent resistance. Strategic violence linked to populist insurgence seems the most practical solution. While not endorsing this approach, Miller reminds us of Engels' dictum that revolutions are ultimately contests of arms.

If the lofty ideals of anarchism for the most part fail to pass the test of rigorous theorizing and effective practive, then why concern ourselves with this political ideology? Miller offers us two reasons: anarchism as critique and the importance of small-scale social experimentation. It is in these two areas that the anarchist tradition lives on. As a mode of critique, anarchism constitutes a potent moralizing force which keeps alive the most radical vision of freedom, exposes the pervasiveness of power relations, draws attention to the negative consequences of state power, and reaffirms an organic communitarian ethos in the face of increasing techno-bureaucratization. Furthermore, anarchist-inspired experimental communities have proven to us that radical freedom and social solidarity can exist, if only briefly, and that the almost hypnotic power of the modern nation-state can be transcended. Miller's survey of anarchist thought serves as a fine introduction to this too often forgotten political tradition. It forces us to rethink several critical questions which plague political societies to this day. Current anarchist thought, which builds its case upon these more classical questions, has developed far more sophisticated arguments which take us beyond the traditional state and revolution paradigm to examine the multiplicity of dominating forces present in our society. Indeed, the most powerful contribution to the contemporary anarchist critique has been developed by none other than Michel Foucault. His analysis of our post-modern age of panoptic control and surveillance, disciplinary technologies, and microphysics of power fits the anarchist critique. What is needed is an equally sophisticated micro-political theory of resistance and renewal.

WAYNE GABARDI

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Conceptions of Liberty in Political Philosophy. Edited by Zbigniew Pelczynski and John Gray. (New York: St. Martin's Press, 1984. Pp. 438. $37.50.)

This book is a collection of 17 essays addressing the thought of 16 writers on the subject of liberty in political philosophy. In their expositions, the contributing authors also address, either explicitly or implicitly, Sir Isaiah Berlin's distinction between negative and positive liberty. Several use Berlin's categories as their themes, concentrating upon ways in which the writers who are their subjects do or do not vindicate the form or substance of Berlin's arguments.

As is well-known, Berlin's distinction between negative and positive liberty rests, most simply, upon a distinction between liberty as noninterference and liberty as self-government. Lurking within the latter category is the potential existence of a rational or "real" will distinct from an individual's expressed will, the content of which may be interpreted and imposed by an individual or group claiming a monopoly on its "correct" understanding. Linking a number of the essays in this volume is a consideration of the degree to which liberty in general, sometimes including the conception of a rational or real will, is in fact compatible with an emphasis upon the value of freedom to the individual, as opposed to its value to society as a whole. In what follows, I shall attempt to draw upon this theme.

The two poles can perhaps be marked by Berlin himself and by Fichte, by a belief in the incommensurability of values as opposed to a commitment to an extreme rational monism. John Gray suggests that although Berlin is highly critical of metaphysical rationalism, his preferred conception of liberty is not an unreservedly negative one. It is compatible with minimal requirements of rational choice and is consistent with a definition of freedom as the nonrestriction of options. Central to Berlin's thought is his acceptance of the competition of incommensurable goods as "an unalterable feature of the human predicament" (p. 344). At the opposite extreme is a vision like Fichte's, foreseeing, as Roger Hausheer writes, an eventual rational harmonization of all human interests and ends. "Men will cease to stretch out for what they cannot attain and so will no longer experience frustration and failure. . . . Men will desire what they can perform and do what they desire" (p. 146). Gray's Berlin and Fichte both grapple with the notion of a real or rational will. In Berlin, it may empower the individual *qua* individual, whereas in the later Fichte it does not.

Between these poles, many viewpoints are represented. James Tully argues that Locke's identification of civil liberty with a law that is constitutive rather than regulative of freedom, completing rather than opposing it, calls into question Berlin's conviction that negative freedom is the sole basis for contemporary "liberal" freedoms. Life in political society is for Locke both natural and free in terms of the fulfillment of human potential. Along related lines, Patrick Gardiner observes that in Rousseau's thought, an individual may act without external pressure (negative liberty) and in response to a genuine desire of his own (positive liberty), yet may still perceive himself as unfree if his desires somehow preclude the realization of ideals at the center of his vision of a worthwhile life. And G. W. Smith argues that J. S. Mill's conception of liberty contains positive as well as negative elements (a view shared by Gray). It includes an emphasis upon the self-mastery necessary to autonomy and self-development, even if it relies upon an optimistic assessment of people's desires for self-development when conditions inhibiting the occurrence of this desire are absent. Tully, Gardiner, and Smith all imply that in the work of the philosophers they are interpreting, the notion of a rational or real will may enhance the value of freedom to the individual.

On the other hand, Richard Mulgan points out that although liberty was important in ancient Greece, it was justified by its value to the community as a whole, rather than by its value to individual citizens. For T. H. Green, freedom depends not simply upon acting according to one's preferences, but also upon the nature of the preferences. Yet, suggests John Roberts, freedom in general is justified by Green for its development of the individual's capacity to contribute to the common good. Even Hayek, according to Norman Barry, often implies that freedom's chief value is in the way it serves society. And his identification of law with liberty on a formal and antirationalistic ground, unlike Locke's similar identification, deprives him of a substantive basis for critiquing restrictive general rules. Moreover, although the final end of communist production for Marx was to secure for individuals time to be utilized according to individual preference (negative liberty?), Andrzej Walicki suggests that true freedom "was not 'negative' in the sense of being aim independent. It was a means for the realization of the final end of history" (p. 239), the development of the new, regenerate species being.

This volume also contains essays on Hobbes by D. D. Raphael, on Spinoza by George Parkinson, on Kant by Charles Taylor, on Hegel by Zbigniew Pelczynski, on Oakeshott by John Liddington, on Arendt by Ronald Beiner, on Rawls by Jeffrey Paul, and on Habermas by Jay Bernstein. The essays are of consistently high quality and induce thoughtful reflection, whether or not the reader agrees with the specific conclusions of individual contributors. Since all essays are at least implicitly informed by issues raised by Berlin's discussion of liberty, their coherence is markedly greater than in many similar volumes in which the essays do not seem related. This book will repay the attention of those interested in epistemology, political thought, and the human condition in general.

EMILY R. GILL

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Lenin and the End of Politics. By A. J. Polan. (Berkeley: University of California Press, 1984. Pp. 229. $22.50, cloth; $9.95, paper.)

First premises shape politics. Political philosophers, whose metier is first premises, come in two varieties: those who erect their first premises into total systems, and those who scrutinize first premises and their consequences. In *Lenin and the End of Politics*, A. J. Polan, a first-rate philosopher of the second variety, scrutinizes V. I. Lenin, a second-rate philosopher of the first variety. Polan offers an "effective history" (p. 53) of Lenin's *The State and Revolution* (1968) by which Polan means (following Gadamer) a hermeneutical reading involving "the reasoned interrogation of tradition *and* the traditional interrogation of reason" (p. 49). Polan concludes that Lenin's "first move," as the philosopher whose premises shape politics both in contemporary

"Marxist" states and in the socialist tradition, "is . . . to eradicate politics" (p. 175).

Polan's book is a meditation on the meaning and possibility of politics in our time, one in which the Gulag and Polish Solidarity both exist. Puzzling over "why the threat of Solidarity was so profound" (p. 5), shaking the foundations of Eastern European regimes, Polan is led to Lenin's "conceptual starting-point" (p. 59) in *The State and Revolution.* That starting-point—Lenin's first premise—concerned the problem of bureaucracy. Lenin contended that bureaucracy was rooted in "capitalist culture" (p. 59) and that, after the capitalist state machine had been "smashed," state functions could be reduced to "simple operations" (p. 59) along lines analogous to the postal system.

Polan's second chapter evaluates Lenin's critique of bureaucracy in light of "an alternative body of work, from Weber to Habermas" (p. 70) which includes such divergent theorists as de Tocqueville, Bukharin, and Gouldner. Polan's assessment is pointed and persuasive: "Lenin's unsophisticated anti-bureaucratism ends up conspiring with the bureaucracy in the maintenance of power" (p. 70). Chapter three extends Polan's interpretation of the "continuing effectiveness of *The State and Revolution* in the Russia of the Gulag" (p. 53) to encompass "how later writers . . . have contributed to a continuing misunderstanding of the world Lenin made" (p. 90). This chapter is a tour de force. Ranging from refutation of various theses about the "degeneration" of the Russian Revolution—through perceptive criticism of the theory of the authoritarian state as developed by Frankfurt theorists Horkheimer, Adorno, and Marcuse—to a compelling analysis of similarities and differences between the American and Russian revolutionary experiences with regard to the rule of law, Polan builds his relentless case against Lenin and Leninist discourse. This reader concurs with Polan's verdict: "[T]he text, in all its moments—libertarian and authoritarian—is guilty of subsequent developments: that is, the features of the authoritarian Soviet regime are present within every line and concept of the text. And it is not just a question of *similarity* between what was written and what later happened: the *cultural* effect of *The State and Revolution* can be suggested as the *causal link* between text and subsequent events" (p. 129). Chapter four is an anticlimactic postscript to the foregoing analysis in which Polan gilds the lily, arguing that "Lenin's cosmology, Lenin's concept of parliamentarism, Lenin's culture, and Lenin's theory of political motivation" (p. 136) were philistine.

Returning to form in chapter five, Polan executes a brilliant exegesis of Sartre's *Critique of Dialectical Reason* (1960). Polan suggests that Sartre "ruptured the [Leninist/utopian] discourse which has previously prevented revolutionaries from grasping the consequences of acting in the world" (p. 191). Here Polan sounds the theme that is his first premise, and his major contribution. From Sartre, Polan derives an appreciation of the relationship between politics and time. Human beings experience the terror of scarce time. Time is the ultimate finite resource and politics is humanity's means of dealing with this fact. Polan writes, "Politics is an ontological failure because it cannot halt the passage of time, the inevitability of change and the passage of generations" (p. 204). Still, politics is all we have. Lenin's " 'crime' " (p. 130) is that he promised a society lacking nothing, a world in which time would have been stopped, thereby rendering politics unnecessary—indeed, intolerable. Lenin's "seductiveness" (p. 206) is that he promised what mortal humans desire. Polan's accomplishments are the repudiation of this promise, and the reminder that, paraphrasing Hannah Arendt, though we must die, we are not born in order to die, but to begin.

JAMES C. FOSTER

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Property and Political Theory. By Alan Ryan. (Oxford: Basil Blackwell, 1985. Pp. iv + 198. $24.95.)

Despite its centrality to the history of political thought, the idea of property and its varous manifestations has received surprisingly little attention from modern scholars. There are a few exceptions to this: Paschal Larkin's *Property in the 18th Century,* Richard Schlatter's *Private Property,* some of C. B. Macpherson's work, Pocock on Harrington and others, Tully on Locke, the NOMOS collection on the subject, a 1979 edition of essays by Anthony Parel and Thomas Flanagan, Lawrence Becker's

Property Rights, H. T. Dickinson's *Liberty and Property*, and recent or forthcoming work by Janet Coleman, Istvan Hont, Donald Kelley, Andrew Reeve, and others. But this list still bears little relation to the importance of the subject, and despite new interest in the eighteenth and nineteenth centuries in particular—when disagreements about property threatened to, and perhaps did overturn western political theory—there are many problems untreated, writers neglected, and questions begged in the field.

One short book could scarcely begin to meet these deficiencies, and Ryan makes no pretense of so doing. Instead, an attempt is made at a philosophical analysis (not a history of ideas) of the relationship between work and property among a number of key western thinkers, concentrating upon how property is justified or criticized, and how it has been related to notions of citizenship. Ryan begins with Locke. Grotius is mentioned once, and, inexplicably, Pufendort not at all, though the latter is certainly as important to eighteenth century thinking about property as Locke—on his own as well as through Vattel, Hutcheson and others.

However, Locke is primarily honored, and secondly Rousseau, because Ryan finds that in these thinkers two modern traditions of thinking about property begin. One of these is the "instrumental" view, in which social institutions, like private property, are justified by their contribution to the mutual well-being of all. The second is the "self developmental" theory, in which mankind is also viewed as God's creature, expressing itself through fulfillment of its duty to God (though this obligation is gradually secularized). After Rousseau, Kant and Hegel can be understood as exemplifying the chief defense of private property from the self-developmental perspective, and Bentham from the instrumental, utilitarian viewpoint. Marx and Mill are then both treated as protagonists in a nineteenth and twentieth century debate about how best to set aside the eighteenth century defense of private property, with Marx attacking largely from a self-developmental, and Mill from an instrumental basis. Ryan's conclusion is an attempt to explain why both Marx and Mill can be understood as having failed to predict the future development of capitalism, and is set in the form of the question, "why are there so few Socialists?," though it might well have been put in the converse, "Why are there so many social democrats?"

Ryan's presentation of modern debates about property in terms of these two traditions is extremely helpful, and commends this book to every student of modern political thought. It is perfectly pitched at the senior undergraduate level and will serve very well for teaching purposes. The discussion of individual thinkers will probably not upset the expectations of most scholars. A longer book would certainly need to say more about the background and connections; i.e., about Aquinas and the medieval natural law tradition, the discussions of money, usury and just price in their political context, as well as the accounts of the origins of private property and its justification given in this tradition. There is far too little about the relation of theories of property to many of the central concepts of political theory, especially liberty (the Physiocrats beg inclusion in such a book).

Despite the gaps the fact remains that the chapters are good introductions to the treatment of the topic by various important writers, and in this sense the book is of greater value for its individual chapters than the sum of its argument. Hence, on Locke, Ryan summarizes current views, on Rousseau he is a good companion to the discourse on inequality and political economy, while the inclusion of Kant helps to stress the latter's importance. A chapter on the Utilitarians makes the telling point that utilitarianism and private property were not inevitably connected in the early nineteenth century, and concentrates on Bentham. On Hegel, Ryan explores the notions of ownership and possession, showing the centrality of these to the structure of Hegel's thought. "Mill and Marx and Socialism" links the two nineteenth century thinkers more closely than is customary, finding them both to be defenders of socialism on the grounds of its greater service to freedom, though the wide gap evident in the positive/negative liberty distinction belies much of this apparent coalescence. Finally, the non-existence of modern systems of voluntary socialism is connected to the general prosperity of contemporary capitalism and to greater popular concern for instrumental, than for other notions of property, as well as to the lack of working-class inclination for self-government.

There is much that is valuable here by way of suggestion and innuendo, but it is a pity that a longer book was not written to explore the topic more fully.

GREGORY CLAEYS

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Ideology and the Urban Crisis. By Peter J. Steinberger. (Albany, N.Y.: State University of New York Press, 1985. Pp. ix + 175. $34.50, cloth; $10.95, paper.)

Ideology—along with pollution, crime, and poverty—seems to be symptomatic of the American urban crisis. Peter Steinberger has attempted to make philosophical sense of its various manifestations in the large body of literature inspired by the misfortunes of the nation's cities. The modest aim of his short book is to link each of several urban ideologies to a particular tradition of political thought and to evaluate the coherence and plausibility of the philosophical material contained in each ideological position.

Steinberger distinguishes ideological works from those of political philosophy or policy advocacy by referring to the purposes for which they were written. An ideological writer does not offer prescriptions for all time, like a political philosopher, but expects them to endure longer than the recommendations of a policy advocate, who addresses specific events and programs. Ideologies can also be distinguished by the degree to which the judgments that they advance are based upon explicit and systematically developed principles. Here again, Steinberger locates ideology midway between political philosophy and policy advocacy.

One wonders how seriously these criteria were followed in practice. For example, Steinberger identifies Daniel Patrick Moynihan's *Maximum Feasible Misunderstanding* as an ideological work even though it deals with a specific federal program. The implication, perhaps, is that the ideological status of a book depends not only on the purposes for which it was written, but on the way in which it is read.

Steinberger outlines three urban ideologies—managerialism, communalism, and possessive individualism. Taking up each in turn, he first identifies the essential tenets of the position, then surveys its philosophical roots, and finally turns to critiques of the ideology's theoretical presuppositions. Particular attention is given to the idea of human nature implicit in each ideology, its theory of moral judgment or practical reason, its notion of political obligation, and its conception of the public realm.

Communalism, the ideology to which Steinberger himself seems most sympathetic, receives the least adequate treatment. With the exception of Alexis de Tocqueville, Steinberger recognizes no important exponents of the communalist position in this country until after the emergence of community action programs in the 1960s. Such important writers as Lewis Mumford and Mary Parker Follett are thereby ruled out of account, and others who belong to the era of community action, like Jane Jacobs and Paul Goodman, are unaccountably ignored. Steinberger relies instead on other proponents of localism, citizen participation, and neighborhood government—most of them less influential than the writers whom he has overlooked.

Steinberger finds the origins of managerialism in nineteenth and twentieth century progressivism, and his account of the position relies heavily upon the writings of selected municipal reformers and urban planners, almost all of which were published over twenty years ago. More recent exponents of managerialism among the practitioners of policy analysis and policy science receive hardly any attention, and as a result Steinberger's version of the position suffers from a kind of arrested development.

Possessive individualism is the ideological position attributed to many of neoconservative writers who have addressed the problems of the cities since the 1970s. Steinberger's account of this urban ideology is based on an analysis of only three exemplary works, but his description of the position is a persuasive one all the same, and his discussion of its Hobbesian affinities is insightful and helpful.

In general, however, Steinberger's effort to uncover fundamental principles is curiously indifferent to fundamental questions. It takes for granted the existence of an urban crisis without considering whether the crisis itself may be an ideological artifact. It asserts that urban politics is "deeply ideological," but never asks what there is about urbanism that evokes ideological responses. It argues that all three of the ideologies described are "insufficiently urban," but neglects to inquire how this came about. Steinberger's closing plea for a more philosophical approach to urban politics would seem to require responses to just such questions.

MATTHEW A. CRENSON

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Marx's Critique of Politics: 1842-1847. By Gary Teeple. (Toronto: University of Toronto Press, 1984. Pp. xi + 303. $35.00.)

Marx's Critique of Politics is a close reading of Marx's writings from his doctoral dissertation on Epicurus through the *German Ideology*. Teeple argues that much of the literature mistakenly characterizes the early Marx as a philosophical idealist or Hegelian, and a political liberal or democrat. These characterizations often lead to the view that there is a break in Marx's development between the early and mature works. By following the development of Marx's problems, Teeple wishes to show that there is a continuity both of philosophical method and political development. Philosophically, the continuity is to be found in a developing method of "critique," while politically there is a progression of questioning that leads from the seemingly liberal-democratic concerns of the *Rheinische Zeitung* articles of 1842-43 to the more radical social and economic concerns of the later writings.

Viewed strictly as a guide to the questions and concerns of Marx's early writings, Teeple's book is careful, systematic, and reliable. Moreover, the broader project is a good one: the literature on Marx has not focused on the philosophical and political continuities of Marx's early development as carefully as it might. The execution of the project, however, has some significant shortcomings. Teeple is clearly very close to Marx, and wishes to defend the continuity and integrity of his thought. But in many cases his defense is not the kind with which most students of Marx—defenders or not—are likely to be comfortable.

For example, on the question of Marx's critical method Teeple correctly sees the method as involving comparisons between some kind of notion of the essence of human nature and society, and existing states of affairs. Thus, he points out that in the *Rheinische Zeitung* articles Marx posited a "real and rational" essence to the state, and used this to criticize the existing German state. But when it comes to showing what distinguishes Marx's critical use of the essence/existence distinction from its long history of use by others, we get little help, even though Marx's distinctiveness in this respect is one of Teeple's major claims. He dismisses secondary literature that treats the early Marx in light of his Hegelian milieu and discourse, but in so doing he describes Marx's critical method in pre-Hegelian, pre-critical (that is, pre-Kantian) terms which seem to support what he wishes to deny—that the early Marx was an "idealist." Problems of clarity are repeated for Teeple's characterization of Marx's critical method after the 1843 *Critique of Hegel*, where he claims that Marx takes a less abstract approach, and begins to find the conflict between essence and appearance within reality itself. Again, this is surely correct, and constituent accounts can and have been given. Teeple does little to illuminate the claim, however. Instead he vacillates between Max's notions of essence (of human nature, society, property, etc.), and empiricist formulations; e.g., "Marx only provides the explanation" of the objective significance of the proletariat (p. 112).

Teeple's thesis regarding the development of Marx's critique of politics displays an insensitivity to current issues in Marxism. He argues that Marx's critique of politics was essentially a discovery that "political relations are but the expression" of historical divisions between the individual's social essence and society (p. 197). Politics is strictly epiphenomenal—something that only occurs in class societies, and will disappear in a communist society. Moreover, according to Teeple, Marx equates politics with the activities of the state, and the state with the interests of the ruling class. Now it is possible to interpret Marx's early understanding of politics in this way, but it is certainly not new, and hardly seems the most defensible or interesting. Politics also has broader meanings in the early Marx: for example, he might be said to be restoring the classical meaning of politics in terms of self-determination. And although Teeple's interpretation produces a "reflex" theory of the state, there is no indication that this is an important and complex issue in Marx.

The conclusion Teeple draws from his account of Marx's critique of politics is especially likely to irritate political theorists. Modern political theory, he claims, is "nothing but the reflection of contradictions in existing liberal democratic political relations" (p. 212). As such, it essentially describes and justifies modern states. In a variation on Marx's "end of philosophy" theme (which is controversial, and by no means widely accepted even among Marxists), Teeple argues that because the problems of political theory only reflect the problems of capitalism, the demise of capitalism would bring political theory to an end (p. 211). This conclusion does little to reflect the range of problems of modern political theory.

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Human Rights, Ethnicity, and Discrimination. By Vernon Van Dyke. (Westport, Conn.: Greenwood Press, 1985. Pp. xii + 259. $35.00.)

Professor Vernon Van Dyke has written a provocative book about an unfamiliar, but important topic—the concept of group or collective communal rights. The principle of nondiscrimination has become almost universal, at least in official documents around the world. The U.N. Charter requires members to promote human rights "without distinction as to race, sex, language, or religion." Yet, says Van Dyke, differentiation among groups may be necessary in deeply divided societies, and this differentiation should not be perceived as discrimination against individuals so long as its major purpose is the promotion of human rights. Simple equality of individuals before the law may be insufficient to assure genuinely fair treatment of all members of society. In fact, says Van Dyke, "some interests of individuals cannot be effectively served unless a right is conceded to the group to which individuals belong" (p. 16).

There may be a collision between the notion of individual rights and the rights of individuals as derived from their membership in groups. In the United States this collision has occurred, taking the form of a great debate over the nature of affirmative action programs on behalf of minorities and women. According to the U.S. Justice Department (circa 1985) most affirmative action programs amount to "reverse discrimination," and are violative of the rights of individuals who are left out of preferential quota arrangements. But even before the Reagan Administration, Nathan Glazer's book, *Affirmative Discrimination* (1975) focused intellectual attention on the risks attendant upon the bestowal of rights upon groups, rather than upon individuals. Preferential treatment for minorities and women in America has been addressed by the Supreme Court which, in spite of convoluted efforts to save some aspects of affirmative action programs, has always avoided recognizing that blacks, women, Hispanics, Indians and other minorities enjoy some form of collective rights to special treatment. On the contrary, the Court has closely scrutinized these programs in order to discern whether they departed from some version of equal protection of the laws. Unfortunately, the Court is badly divided over the meaning of equal protection in the face of numerous public and private programs to give special opportunities to minorities and women. If the Supreme Court followed the Van Dyke argument in favor of some sort of collective group right attached to disadvantaged minorities, it could greatly clarify the precedents in this important area of public policy. However, American individualism is deeply rooted in our political ideology, as well as in our constitutional law.

Van Dyke is probing into a very sensitive area in liberal political theory when he builds his case for group rights. Only the principle of majority rule exemplifies some sort of collective right in liberalism. In spite of theory, Van Dyke demonstrates that the law of many countries accepts certain ethnic communities as "collective juridical personalities" (p. 15). His choices are selective: Belgium, Cyprus, Fiji, India, Malaysia, and South Africa. These countries, unhampered by liberal preconceptions about rights as exclusively individual in nature, have established some form of group rights, usually in the interest of the protection of communal differences of language, religion, or culture. Special political arrangements for minorities were created in all of these nations, although few would regard apartheid in any of its incarnations as giving rise to group rights.

The best case for special protections for groups can be made for indigenous people: the Indians of the United States, the Maoris of New Zealand, the "scheduled tribes" of India, the various Indian groups of South America, and the like. These people have usually been overrun by members of a more advanced civilization, and require protections against the further loss of their lands, their health, their culture, and their very lives. However, it is difficult to treat in the same way the South African homelands policy, which creates officially imposed, white-defined boundaries upon the citizenship, livelihood, property, and physical movement of various black peoples. Van Dyke does not defend South African policy, but one wonders what purpose is served by including South Africa as an example of group rights, since the government has done everything possible to avoid allowing black groups to organize to present their special wants and needs.

Political scientists should have been aware of collective rights before Van Dyke prepared this book, but the topic has been largely ignored. The first modern work in this field was Inis L. Claude's *National Minorities*, written in 1955 from the perspective of international relations. In 1960 J. A. La Ponce expanded the treatment by concentrating upon legal and constitutional provisions which protected minorities. My own book, *Minority Rights* (1983) covers the ground in greater detail, and develops a definition of minority rights. However, Van Dyke raises the most profound theoretical questions about the implications for individualism of any theory of group rights. The final 30 pages of his last chapter are the fruit of his extended reflection upon this complex and misunderstood issue. In it Van Dyke proposes nine different criteria for assessing the appropriateness of a group rights claim. In an age when Basques, Palestinians, Sikhs, Tamils, fundamentalist Moslem groups, and other active minorities have projected themselves into the headlines, political scientists must take note of the growing significance of claims of group rights. The political salience of this topic is growing. Some readers may not entirely agree with this radical reassessment of liberal theory, but events have overtaken the theory.

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POLITICAL ECONOMY

The Politics of Production: Factory Regimes Under Capitalism and Socialism. By Michael Burawoy. (London: Verso, 1985. Pp. 272. $8.95.)

Michael Burawoy's *The Politics of Production* is a learned and provocative book which, nevertheless, falls far short of fulfilling its promise. This is perhaps inevitable given its ambitious goal, which is nothing less than an attempt to formulate a general explanation for, as the author puts it, "the intervention of working class in history"—an attempt to specify how and under what conditions the working class has made its presence felt as a political actor. In doing so, Burawoy wants to rescue the working-class from those on the "left" who would either assign it an all-powerful, messianic role, or ignore it entirely and replace it with sundry new movements like environmentalism or feminism. Burawoy's objective, then, is not only to see the working class as an active creator of its collective life but to specify the forces which determine why, when, how, and with what effects it does so. In his view, these forces are related in one way or another to what he calls the "politics of production."

Burawoy is, of course, not the first scholar to attempt to explain the complexities of the politics of the working class. He is the first, however, to attempt a general systematic analysis of such politics that is based on the labor process and social relations in production. The linchpin of this analysis is the concept of the factory regime. For Burawoy, the factory regime has various political effects. The political apparatuses of production and the various institutions that shape and regulate struggles at the workplace are the key to understanding the activity of the working class. He goes so far as to claim that "variations in factory regime are *sufficient* to explain working-class reformism in England and a revolutionary movement in Russia," and implies the same explanatory power for it in relationship to other cases of working-class politics.

The book is an exploration of the different historical forms of factory regime, the determinants of such regimes, and most importantly the political effects that each type of regime may be expected to have on the working class. A series of case studies on England, Russia, the United States, Japan, Hungary, and Zambia puts the theory of factory regimes to the test in a wide variety of historical and political-economic settings. In my view, the theory fails the test, though we learn a great deal that is valuable during that journey.

The journey is endlessly fascinating. Along the way, for instance, Burawoy offers a brilliant discussion of the labor process in capitalism by way of an analysis of the work of Harry Braverman and his critics, and then extends that discussion into one of the most illuminating considerations in the literature on the capitalist mode of production (CMP). He shows, quite convincingly, that the CMP may be distinguished not by the separation of conception and production (as in Braverman), but by the relatively invisible extraction of surplus from the working class. There are also fascinating materials on the unique qualities of the CMP in the United States; short histories of material production in the U.S., England, and Japan; and some of the particulars of work life in places as diverse as pre-revolutionary Russia, Japan, and Zambia in the pre- and post-colonial period.

With all of that said, I am not convinced that the politics of production, and especially the concept and theory of factory regimes, explains as much about working-class politics as Burawoy claims. This is the case because what Burawoy has done in this book is not so much to construct a theory of the politics of production as to provide a diverse set of analytic categories, each illuminating some corner of reality, yet not holding together in any sort of discernible whole. We are told, for instance, that factory regimes are determined by a complex mixture which includes the labor process, enterprise relations to the market and to the state, the mode of reproduction of labor power, and the place of the national economy in the international economy. Yet we are offered no hint of what weight to assign to each, or of how the parts systematically interact so that findings are generalizable beyond the single case being described. Burawoy's taxonomy of factory regimes, what should be the heart of the analysis, moreover, has a curious ad hoc quality to it as factory regimes proliferate and transform themselves without any comprehensible and systematic conceptual apparatus to help us to understand this multiplication and change. The reader, or at least this reader, is in the end unable to discern what it is that is most critical to Burawoy in the many complex processes that shape these regimes, guide their activities, and determine their effects on the working class. Strangely enough, while I learned a great deal about the labor process, the reproduction of the working-class, the technical division of labor, the forms of labor control, and the like, I cannot say that I know more in a systematic and theoretically parsimonious way about the politics of production and of the working class than before I read the book.

Having said that, let me reiterate that the book remains endlessly fascinating, replete with surprising information and provocative forays into some of the most compelling issues in the social sciences. In this regard, and despite its many problems, I would recommend *The Politics of Production* to a wide audience. Even in its failure, we see the bright promise and aspirations of the best of the social sciences.

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Modeling as Negotiating: The Political Dynamics of Computer Models in the Policy Process. By William H. Dutton and Kenneth L. Kraemer. (Norwood, New Jersey: Ablex Publishing, 1985. Pp. xiii + 261. $39.50.)

The Dynamics of Computing. By John L. King and Kenneth L. Kraemer. (New York, N.Y.: Columbia University Press, 1985. Pp. xii + 283. $40.00.)

Against the background of Orwellian nightmares portraying computers as efficient instruments of government oppression and control, these two books examine the routine realities of computer processing and computer-based modeling in local government decision making. One of the studies, *Modeling as Negotiating,* argues that computer models can and actually do promote negotiation and compromise among pluralistic interests in a democratic policy process. This general finding emerged unexpectedly from the authors' case studies and surveys, and it led them to formulate a "consensual" view of modeling as an improvement on the more traditional rationalist, technocratic, and partisan perspectives that dominate the literature. The other study, *The Dynamics of Computing,* surveys local government computing policies and practices during the period 1974-1979 in 40 American cities and 16 cities in nine Organization for Economic Cooperation and Development (OECD) member nations. It finds that computer utilization creates different types of problems and benefits depending on a government's prior experience with computers and its current goals and expectations. Management policies involving technological development, managerial structures, and "socio-technical interface" (e.g., user training) are not panaceas for problems that keep changing with each advancing stage of computer application in the policy process.

Of the two books, *Modeling as Negotiating* is by far the superior work in terms of study design, conceptual clarity, substantive findings, and provocative ideas. The authors base most of their analysis on four case studies (Irvine, CA; San Diego, CA; Tulsa, OK; Anaheim, CA) of local governments that used computerized fiscal impact models in making land-use decisions during the late 1970s. They also conducted a mail survey of principal analysts and model users in the 21 local governments that employed one of the four fiscal impact models examined. (Fiscal impact models calculate the likely public costs and revenues generated by alternative development patterns associated with proposed general plans, major rezonings, housing developments, proposed freeways, new tax rates, etc.). Departing from most earlier studies, the authors focus on the politics of implementing these models in the policy process, rather than on technical aspects of model development. Their study was guided by two broad questions: Who controls how these models are used?, and, Whose interests are served by them?

Addressing the question of control, the authors conclude that "[n]o one individual or group systematically and uniformly dominates all activities and decision processes" (p. 157). They argue that modeling is "labyrinthian" in the sense that "there are multiple actors whose participation varies from stage to stage, and within stages" (p. 159). The cases clearly demonstrate that modeling "is not simply an activity of the model builders and consultants" (p. 160), and that elected officials, planners, bureaucrats, private developers, and community activists all play important roles in shaping the assumptions, data, and alternatives fed into the models. In adapting models to local circumstances, these actors find their attention increasingly fixed on the computer map rather than the actual territory of their disputes. Model-building unfolds as an iterative, n-person bargaining game and becomes democratized as many hands, in effect, tap at the keyboard to reprogram assumptions, set parameters, and execute runs. According to the authors, this kind of pluralistic "modeling as negotiating" tends to promote consensus as an unintended byproduct of the modeling process itself, and not primarily as a result of the model's outputs. This "consensual perspective" on modeling is offered both as an empirical generalization of their findings and as a normative guide to planners and consultants. They write: "When the modeling process is carried on as an isolated technical exercise or as an obvious partisan maneuver, the models tend to lack relevance or credibility, respectively, in the decision-making process" (p. 211).

Regarding the question of whose interests are served by modeling, the authors conclude that fiscal impact models "appear to systematically advantage those community groups and interests supportive of exclusionary, fiscal zoning policies. Lower-income renters and their advocates are disadvantaged" (p. 197). They worry that routine incorporation of such modeling can lead to the "automation of bias" in local government decision making (p. 215).

I found *Modeling as Negotiating* to be a valuable and insightful study of computer modeling in the policy process. The case studies almost stand alone as interesting and well-told stories of the politics of development. (Sample quote from an Anaheim developer commenting on a computer model's incomprehensibility: "[I]t was so complicated that it was unintelligible to non-experts. It was like handing a copy of *Gray's Anatomy* to a housewife to deal with her kid's cold.") Not only is the book an excellent contribution to the scholarly literature of political science and public administration, it also offers an open harvest of practical advice to those planners and consultants who have computers and will travel in the domain of politics.

The authors of *Modeling as Negotiating* make an interesting comment that is relevant to an evaluation of the second book under review: "Ironically, computers permit incomprehensible models to be built, in that computer programs can handle more variables, calculations, and data elements than any single person could fully comprehend" (p. 173). Computers also permit incomprehensible books to be written, particularly when the computer's numerical output is virtually dumped in undigested heaps on page after page of abstract prose. Despite the authors' worthy scholarly purpose, *The Dynamics of Computing* suffers from near-incomprehensibility in several chapters precisely because of human failure to select, organize, and interpret the machine's abundant output in theoretically meaningful terms. To illustrate, in one chapter three short paragraphs are devoted to an interpretation of two tables containing 346 Pearson correlation coefficients (pp. 119-125). Similar occurrences of statistical mountains accompanied by verbal molehills are found throughout the book. Relationships involving many interesting variables (e.g., degree of data linkage, centralization, and size of EDP staff) are explored using correlation and regression analysis, but the authors rarely descend to the level of discrete linkages to make theoretical sense of their findings. They prefer to generalize in terms of such "variables" as "management policies," "computing environment," "benefits," and "problems," which are dimensionless vacuities lacking any metric or meaning beyond their service as category labels.

The sampling design of the study appears flawed. For unstated "statistical reasons" (p. 16), only 40 of the original 400 American cities surveyed were included in this analysis, 20 at the high end and 20 at the low end of six policy continua measuring such things as a city's degree of automation and degree of decentralization of computing resources. This selectivity undermines any generalization of findings to the total population of medium-sized and large cities. The study's analytical methodology is not articulated or defended in any one place but becomes manifest only as the book unfolds. Factor analysis is employed in analyzing certain categories of variables, but not others. Partial correlations are shown controlling for city size but not for other variables. Regression analyses were performed, but it is not clear what independent variables were specified in the regression equations. By conventional standards of scientific reporting, the table reporting the regression results (Table 7.1) is almost incoherent.

The authors aspire to answer the question of "what computing, broadly conceived, looks like *in vivo*" (p. 15). In fact, they examine not a single "living" local government system but only scattered traces in the verbal reports of survey respondents. The only "dynamics" in this study are applications of what the authors call "stage theory" to the cross-sectional survey data. Such theories certainly seem necessary in our efforts to track and comprehend the computer revolution, but the particular theory expounded here is rough and sterile. (Here is a sample "implication" derived from the "theory": "[C]omputing development is highly dynamic, but ultimately is controllable through concerted and informed policy action" [pp. 205-206].) On the whole, this is not a book to be recommended.

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What Is Political Economy? A Study of Social Theory and Underdevelopment. By Martin Staniland. (New Haven: Yale University Press, 1985. Pp. xi + 229. $18.50.)

Martin Staniland attempts to answer the question posed in the title of his book, plus a number of other subsidiary inquiries, the most important of which pertains to the success of competing theories in explaining economic underdevelopment. Both questions are important and deserve reflective responses. His task is great; for many and varied approaches and theories now claim the highly fashionable label of "political economy." While Staniland's essay of little more than 200 pages is brief he manages to provide a good deal of informative and thoughtful concern in a fair-minded manner.

The range of political economy is examined primarily through a series of critiques of writings by mostly well-known contributors to the field. These authors and their analyses are variously classified as examples of "The New Political Economy," "Politicism," "International Political Economy," and "Marxism." Although I am not a student of underdevelopment, I am both amazed and a bit chagrined at the absence of Peter Bauer's name and work. Those who are included seem eminently appropriate as leading proponents of the four schools of political economy.

According to the author, current concern over the relationship between economics and political science is justified and not just another fad. Appropriate relationships between the two disciplines have been at issue for more than 200 years. As Staniland sees it, most theorists have resolved the hiatus by making one or the other dominant—as is true of Marxism and what he terms "politicism." In the former instance, politics is simply a product of or determined by economic relationships, while in the latter, markets and economic behavior are the creatures of political, rather than class interests. Some theorists have, of course, seen varying forms of interaction between politics and markets and, therefore, between economics and political science. The "New Political Economy" or "Public Choice" is considered by the author to be but the latest variant in the notion that economics dominates political science. His position seems somewhat confused in at least two respects: first, the new political economy is considered on a par with much more specialized theories about international dependence when, as a matter of fact, the former is a generalized attempt to explain virtually all human behavior—not simply that of relationships among economies and states. Secondly, the new political economy does not claim the primacy of economic motivations, incentives, institutions over other motivations, etc.; instead, it maintains that the tools of microeconomics can be usefully employed in understanding and predicting human choice, whether found in market or non-market institutions.

Marxism, on the other hand, genuinely insists that economic considerations dominate all others. One of the reasons why I, personally, have never been persuaded by Marxism and its various manifestations in international political economy stems from this basic consideration. Ironically, the new political economy places considerable credence in non-economic factors and, ironically too, the new political economy stresses the substantive primacy of political incentives and institutions and might therefore fall in his category of "politicism." Furthermore, Staniland does not seem fully to recognize the new political economy as a theory of political failure. Although political institutions and choices are viewed as dominating the private economy, their workings are such as to produce collective inefficiency and inequity not only in the polity but in markets. Robert H. Bates, whom the author considers the most able of the new political economists, surely agrees with his proposition as does Peter Bauer.

The author's understanding of these matters seems flawed because he makes no effort to examine the content of public choice in the area it has explored the most, namely, the internal functioning of western democracy. Scholars working in public choice will find nothing on public choice, proper; i.e., theories pertaining to voting, coalitions, political competition, bureaucracy, campaign strategies, and the supplies of public goods—all staples of the journal *Public Choice*. Had these areas been explored the author would have discovered that public choice is hardly as homogeneous as he appears to believe. Instead he would have found not only competing theories in each of the above-noted areas but, more importantly, vigorous dissent over basic methodologies. The Buchanans, Tullocks, Rikers, Tollisons, McKelveys, Plotts, and others are as much divided as are other social scientists over the appropriate roles of mathematics, experimentation, polls, etc. Public choice has its positivists and Austrians, its game and exchange theorists. How could it be otherwise? Accordingly, Staniland informs us more on the subtitle than the main title of his essay; we learn more about efforts to explain underdevelopment than we do about political economy and its several schools.

Despite these oversights and possible misunderstandings, Staniland makes a useful contribution to political theory. He raises an important epistemelogical question about the applicability of modern political economy (ies?) to problems of underdevelopment. While Public Choice has yet to make a major assault on the matter, other theories purporting to explain underdevelopment seem hopelessly ideological. Perhaps, Staniland's small book will perform some consciousness-raising among political economists.

William Mitchell

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The Disabled State. By Deborah A. Stone. (Philadelphia: Temple University Press, 1984. Pp. xiii + 241. $24.95.)

This is a book about the meaning of disability policy, but on a broader level it is an account of the manner in which societies seek to rationalize difficult distributive decisions. Deborah Stone's thesis in *The Disabled State* is that the disability category constitutes a moveable boundary that is manipulated to regulate access to social welfare benefits. Disability is a socially defined category of welfare that, unlike old age or childhood, can justifiably be defined in a variety of ways. Stone's intention in this research is to illuminate the manner in which disability has developed conceptually, and to explain in that light the intellectual basis of disability policy in "the modern welfare state" (p. 3). She contends that her approach will aid in explaining some facets of disability policy heretofore inadequately treated. Among her major questions are: Why have disability programs grown in size and cost in western democracies?; Why has disability determination been increasingly medicalized over time?; Why, even in the presence of medical certification of the disabled, do definitions of what constitutes disability vary among nations?; Why are citizens beginning to support cuts in disability programs?; and, Why do the costs of and enrollments in disability programs in western democracies increase as unemployment increases?

The author is evidently well-read and imaginative: she seeks to answer these and other questions by drawing upon an eclectic assortment of materials, among them case studies of policy development in Great Britain, Germany, and the United States; empirical health and social policy studies; interviews with physicians and officials of health agencies; transcripts of hearings and meetings, mainly in the U.S. House and Senate; newspapers and other materials from the popular press; and, in the final chapter, speculative scenarios.

Stone's argument is anchored by her contention that the determination of who shall be granted access to the need-based versus rule-based distributive system is the major distributive decision facing societies. Entry into the need-based distributive system guarantees that, on some level, an individual will be taken care of by society. Disability determination presents a difficulty in this case because it may be feigned by malingerers who wish to free-ride. Societies seek some means for screening disability claimants to discourage those who would take advantage, and medicalization is one answer to the dilemma. However, medicalization is not a perfect solution; disability is difficult to measure, and disability determinations are subject to much debate.

Stone's discussion, in Chapter 2, of policy developments in England, Germany, and the United States indicates that virtually all forms of welfare determination are subjective, and that disability is the most subjective among the lot. She casts disability as a policy tool developed for regulating the mobility and composition of the labor force. English poor-law policies were restrictive, reflecting a perceived need to limit the mobility of the poor; German social-insurance policies of Bismarck's era are portrayed as having been designed in part to gain acceptance for the state and to maintain class distinctions. (The Germans distinguished between disability for white-collar workers as opposed to blue-collar workers.) Stone notes, rightly, that this was a much a reflection of Germany's rapidly developing industrial and urban economy as it was a desire to keep classes distinct for undemocratic reasons. Her case study of the development of disability policy in the United States, to which she devotes the majority of her text, tells a story of increasing pseudo-rational policy development and ultimately punitive treatment of even the truly disabled.

Stone also gives some space to the problems and politics of disability determination and measurement. Although developments in medical science since 1900 have increased that discipline's legitimacy, it remains virtually impossible to arrive at an objective method for determining disability. Thus there are conflicts in the disability determination process. Claimants and their personal physicians are at times placed in an adversarial role in dealing with agents of the state, be they administrators or physicians. Politicians and other upper level policy makers also face a dilemma: they must find ways to curb disability costs without appearing to be ogres. Each side tries to use medical science as the basis for their arguments, politicizing the role of physicians in the disability process and giving them tremendous power over the distribution of a public good. The process of disability determination is thus very subjective and political. Claimants seek expansion and policy makers must try to limit their access in order to ration public resources.

This conflict leads to increased attempts to make eligibility categories within disability determination more rigid through ajudication and other administrative acts. The advantage gained by covering the bases may be outweighed by the concomitant loss of administrative discretion in disability determination. Stone argues that the rigidity of definitions of disability may in fact contribute to the growth of policies and the perceived crisis: as people come to understand a given misfortune to be a reason for claiming disability, and as the number of misfortunes considered disabling increases, there is an increase in the number of persons who consider benefits an entitlement. Once people are on the disability roles, it is difficult to get them off. Programs grow, and policy makers are thus faced with the unpopular task of rationing access even to the truly needy.

Stone presents a number of interesting ideas in this book. Her notion of social boundaries is a useful contribution to the study of eligibility determination, and her discussions of developments of welfare policies in England, Germany, and the United States, despite their being focused on disability, are thorough and should prove useful to students of welfare policy. This research also constitutes a useful contribution to the burgeoning "crisis of the welfare state" literature. The author has clearly thought long and hard about the meaning of disability policy and its attendant politics and presents a compelling case for her interpretation.

This is not to say that there are no problems with the book. First, the prose is at times tedious and so serious as to verge on pomposity. For example, "The intellectual problem for the scholar of social policy, therefore, is not simply to ferret out the existence of surrogate measures but to understand how such measures distort social reality and why particular distortions come about" (p. 117) and, "The transformation of the idea of inability to work into the clinical concept of impairment is a fascinating intellectual story" (p. 109). What is a "fascinating intellectual story" and how does it differ from a good tale? The research design is also at times bewildering: she introduces her research with a discussion of the role of disability in the "modern welfare state" (p. 3); presents data showing program growth in seven western democracies (p. 8); moves to a discussion of policy developments in Great Britain, Germany, and the United States; and then devotes the vast majority of the remainder of her text (Chapters 4 through 6) to a discussion of U.S. disability policies. Her rationale for studying the U.S. concept of disability is that "it embodies the clinical concept of disability *in extremis* [Stone's italics] and so presents a magnified version of the central definitional problem of all disability programs" (p. 14). Even though Stone has chosen to grapple with a difficult object of inquiry (the concept of disability) and is necessarily writing at a highly abstract level, I am not convinced that the concept of disability in place in the United States is necessarily comparable to that of, say, Finland, and would have been more swayed by her arguments had a more consistent methodology been applied. Despite these criticisms, this is an important book for students or researchers with an interest in social policy and suggests important theoretical work to come.

CHARLES J. BARRILLEAUX

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The Share Economy: Conquering Stagflation. By Martin L. Weitzman. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 167. $15.00.)

Martin Weitzman has written a provocative and original book that outlines how modern capitalist economies can overcome stagflation. What is required, Weitzman argues, is to move from a fixed-wage contract system to a system where employees' compensation is tied to a share of company profits or revenues.

Weitzman traces the roots of our current stagflation to the institutional features of the post-war economy, particularly the rise of wage contracts which tend to fix labor costs in the short run regardless of what happens to the firm's profitability. This feature plus the existence of many large monopolistically competitive firms, which have some power to set prices, is a recipe for stagflation. Consider what happens when there is an unexpected drop in demand for a firm's product. The firm typically reacts by cutting output and laying off workers so as to maintain its monopoly price and profits. Lowering its price would reduce its profits since its unit labor costs are fixed in the short run. The predisposition of the system is for this contraction to be spread across the whole economy because laid off automobile workers cannot afford to buy washing machines and so on.

This tendency towards stagflation in capitalist economies was observed by Keynes, who proposed that government spend to maintain a high level of aggregate demand. To Weitzman the Keynesian solution was a "dazzling digression from the main route to economic prosperity" (p. 54) because it did not tackle the fundamental problem of wage rigidity. Indeed, the full-employment promise of modern welfare states convinces people that contradictions will be brief, and removes the potentially deflationary effects of recessions. Wages and prices increase even during economic contractions. Hence stagflation. Weitzman argues that a share system would permit labor costs to fall in response to unanticipated declines in demand without producing significant declines in output and employment, and would encourage expansion without creating inflationary tendencies in the economy.

In a share system, large firms will behave quite differently than is currently the case. If there should be an unexpected fall in demand for an industry's product, the firms will now lower price to preserve their markets, knowing that labor costs will also fall sufficiently to permit profits to be made. The adjustments will be on the income side for labor rather than via layoffs and unemployment for some workers. Moreover, firms will actively seek to expand output because they will know that labor costs will eat up only a fixed share of any additional revenues. So even with a decline in the price of the product some profits will still be made. Thus, share firms will forever be "cruising around like vacuum cleaners on wheels, searching in nooks and crannies for extra workers" (pp. 98-99).

According to Weitzman, there are many other good effects of a share economy. For example, with many firms actively seeking additional hires, firms will have to compete on working conditions to attract labor. But can a system with all these good properties work in practice? Is it politically feasible? Weitzman recognizes that the main opposition to a share system would come from high-seniority workers who are currently earning good wages, and who enjoy reasonable security of employment. In the transition to a share system such workers would see their relatively high pay levels fall as they shared their portion of the revenues with additional hires, and also might experience greater variability in their earnings as the economy continued to experience structural changes. To persuade these workers to accept a share system Weitzman recommends an educational campaign "to infuse a sense of social responsibility into the collective-bargaining process" (p. 128) and the use of tax incentives (workers on share contracts would pay lower taxes than those on wage contracts). To qualify for the tax benefits workers must agree not to prevent additional hiring by the firm they work for.

One cannot help being impressed by the power and simplicity of Weitzman's idea. I do believe he underestimates the degree of earnings variability that might obtain in a share economy increasingly integrated into the world market. He also underestimates the likely extent of opposition from a large segment of the workforce who have been relatively insulated from the employment effects of recessions. Expecting a large proportion of the labor force to accept more variation in their income, so that five or ten percent of unemployed job seekers can have a job, may be assuming more altruism than our society has conditioned us to possess.

Nevertheless, advanced capitalist economies are desperately seeking some solution to the current economic doldrums, and Weitzman's idea has the desirable quality of promising much. I suspect that the ideas contained in this book will find a friendly welcome in many social democratic parties (including the Democratic party in the U.S.) and may well form the basis for a reform of many societies' industrial relations systems.

LEON GRUNBERG

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AMERICAN POLITICS

Politicians, Judges, and City Schools. By Joel S. Berke, Margaret E. Goertz, and Richard J. Coley. (New York: Russell Sage Foundation, 1984. Pp. xxii + 278. $28.00.)

Beyond Busing: Inside the Challenge to Urban Segregation. By Paul R. Dimond. (Ann Arbor: The University of Michigan Press, 1985. Pp. xii + 411. $29.95.)

These two books have a number of things in common. Each contemplates educational reform in the interest of equalizing educational opportunities. In each, the desirability of reform is taken substantially as a given. These narratives begin posterior to such considerations, and hence rest upon a considerable prior body of assumptions and values. The same can be said of all writing, of course, but because these prior questions of value have been so controversial in recent American history, and because much disagreement persists over them, I am afraid that one's reaction to each of these books will depend more on one's ideology than upon what either actually has to say.

Berke and associates make a cogent statement of the problem. There are great inequalities in the taxable wealth of school districts, in the educational problems that they face, and in the amounts that they spend in support of educational programs. That local school districts are unequally endowed with taxable resources comes as no surprise, nor that there are tremendous disparities in expenditures per pupil among school districts.

A further source of inequality among districts is the concentration of high-need students in urban school districts, and the more intense competition for tax dollars that characterizes metropolitan areas. When tax bases are compared, for instance, the "big five" districts of New York—New York City, Buffalo, Rochester, Syracuse, and Yonkers—do not appear to be disadvantaged given their relative abundance of commercial tax resources. Nevertheless, these districts have much heavier concentrations of students who make particularly high demands for educational services—students who require bilingual instruction, for instance, and those with educational and physical handicaps. Also, owing to the fact that the provision of routine services is more expensive in heavily populated areas, these districts find it more difficult to generate additional revenues locally to finance programs for high-need students.

This book is informative on a number of grounds. There is a very complete discussion of the types of programs that states have traditionally used to attempt to alleviate financial inequalities between districts. The state educational aid program in place in New York is analyzed in depth, and potential alternatives are exhaustively explored. The last chapter of the book offers a prototype equalization program that would substantially equalize financial support for educational programs across districts in the state.

All of these considerations make the book quite valuable, but any further promise is lost when, after constructing quite a case for reform, the authors substantially fail to avoid the rocks upon which previous vessels have foundered. The problem has never been an inability to build a case against school district financial inequality. As the authors note, most states have moved against these inequalities by adopting foundation and power-equalizing programs. However, as they further note, such programs have been ineffective in producing any lasting equality among districts (pp. 132-134). Foundation levels are set so low that wealthier districts can and do spend well above the mandated level; and power-equalizing schemes are always flawed by the compromises that are necessary to persuade wealthier school districts to agree to them. The price of wealthy district cooperation is always hold-harmless provisions that protect these districts from reductions in state aid. Once these compromises are built in, equalizing programs do comparatively little equalizing.

The prototype plan offered by the authors promises to overcome these difficulties and actually eliminate interdistrict disparities in New York. They are eventually driven to admit, however, that the cost of this plan is so great that it is difficult to imagine its adoption. Additionally, the cost of equalization produced by a revision in the state-aid formula is borne most heavily by wealthy suburban districts whose funds would be reduced. Their approval and subsequent political passage of such a reform could most probably be had only if hold-harmless provisions were built into the plan. The reader will note that at this point our vessel, like so many before it, has drawn perilously close to the shoals. The authors finally consider revisions to their program, but these require a reduction in the level of state aid and serious damage to the plan's ability to actually equalize across districts. In sum, this is a book that lucidly discusses the issues in school finance and provides valuable reference material, but which offers no new insight concerning the political problems that have traditionally confounded the best efforts of reformers.

Paul Dimond is an attorney rather than a social scientist. Where the work of Berke and associates is clearly meant to be in the tradition of the literature of policy analysis, Dimond's book has more the quality of an extended memoir. He refers to the principals by their first names (Bill Lamson, Nate Jones, and Lou Lucas), makes fond references to their personal traits, and generally pays no attention to scholarly canons of argument and support. This is one man's perspective on the important litigation over racial equality in public education of the sixties and seventies (though a very interesting perspective it is), and his statement of personal principle and commitment.

The significance of this book should not be underestimated, however. It cogently discusses the notable cases of this twenty year period—*Swann v. Charlotte-Mecklenberg*, *Keyes v. Denver School District*, *Milliken v. Bradley*, *Columbus Board of Education v. Penick*, *Dayton Board of Education v. Brinkman*, and *Delaware Board of Education v. Evans*. The development of precedent in the school desegregation cases is treated quite thoroughly, and there is much elucidation of the processes by which the federal courts arrive at general rules of law, apply them, and choose between them. Those who are interested in constitutional law and judicial policy making will find this book essential reading.

The fact that Dimond does not take a traditional scholarly approach to his subject is a strength as well as a weakness. Political scientists should expose themselves to different approaches to things political every now and then, and particularly when there is a good deal to learn about the texture and structuring of federal litigation. Those who have participated as consultants in federal school desegregation litigation will appreciate Dimond's understanding of the role of social-scientific data in the building of such cases, and the way in which social-scientific arguments are linked to legal reasoning. Social issues are increasingly the subject of litigation in our society, and this book can inform social scientists about the underlying strategies and structures that are not always apparent in published records and opinions.

In considering this book, it should be understood that Mr. Dimond is a man with a purpose. His purpose is to decry the fact that ours is still a racially divided, and in many ways racist, society. He pursues this purpose by recounting this litigation, much of which he participated in directly. Though this litigation was frequently superficially successful in that it resulted in court-ordered integration, blacks and whites are still isolated from each other in their neighborhoods and schools. A proximate cause is the narrow focus of the federal courts in dealing with these questions. This proximate cause, however, rests on an underlying complacency about racism in this country. Racial isolation persists, Dimond says, because "it is easier to feign ignorance and claim innocence than to confront the issue and accept responsibility" (p. 400).

This review began by asserting that there was much that these books have in common. It should end by noting that they also share a common omission. In each book, the structure of educational governance in the United States is frequently implicated. Clearly the source of financial inequality among school districts is the fact that they bear a great deal of the burden for financing their own educational programs combined with the compartmentalization of taxable resources within school district boundaries. Just as clearly, one of the chief reasons for segregation in education is the ability of local populations to create autonomous school districts, and to protect district boundaries by various means against penetration by racial and ethnic minorities. These districts then stand as havens for middle-class whites who wish to avoid court-mandated desegregation within central-city school systems. Given these facts, it is curious that neither author makes any prescription for altering the locally autonomous structure of public school governance. It would seem at the very least that this fragmentation of educational government provides the arenas and points of access that permit parochial interests consistently to frustrate attempts to realize the promise of equality that is integral to our national ideology. In a more general sense, this political atomization is both symptom and cause of a persistent inability to conceive and execute any enduring public purpose.

GREGORY WEIHER

University of Houston

The Nations Within: The Past and Future of American Indian Sovereignty. By Vine Deloria, Jr. and Clifford M. Lytle. (New York: Pantheon Books, 1984. Pp. 293. $10.95, paper.)

The Bureau of Indian Affairs. By Theodore W. Taylor. (Boulder, Colo.: Westview Press, 1984. Pp. xiv + 220. $25.00.)

"The power to tax members and non-Indians is surely an essential attribute of . . . self-government." With those words, the U.S. Supreme Court, just this past April, once again upheld American Indian tribal sovereignty. The Court held that a tax levied by the Navajo Tribe (in its "role as a sovereign," as distinguished from its "role as a commercial partner") on an oil company's reservation leasehold and its gross receipts was valid, even though the tax had not first been approved by the U.S. Secretary of the Interior (*Kerr-McGee Corp. v. Navajo Tribe of Indians et al.*, Slip Opinion, *U.S. Reports*, April 1985, p. 5).

There are about a million and a half American Indians, less than half of whom still live on reservations. But wherever they live, those who are members of the more than 300 recognized tribes are different from any other U.S. ethnic minority: their tribes—unlike the League of United Latin American Citizens or NAACP, for example—are units of government. Our federal system, then, is made up of the national government, the state governments (and within them, of course, local governments), *and* tribal governments.

Theodore Taylor's book on the Bureau of Indian Affairs (BIA), and the book of Vine Deloria, Jr. and Clifford Lytle on Indian sovereignty are both at their best in chronicling the history of federal Indian law and policy, with Taylor especially detailing well and interestingly the history of the BIA—the loved-loathed agency which, until anti-poverty days, administered just about the entire extent of federal interest in Indians and tribes. Through the years, federal Indian policy has fluctuated from one of conquest (up until 1871), to one of individual assimilation and the allotment of tribal lands (1871-1928), back to an era of tribal reorganization and the strengthening of tribal governments (1928-1946), then again to one of "complete integration" along with the termination of governmental recognition for some tribes (1945-1961), and finally to a policy of self-determination and tribal sovereignty (1961-present).

More recently, new tribal assertiveness has produced legal clashes with state and local governments, as well as with non-Indians and business corporations, over such issues as hunting and fishing, zoning, bingo gambling, cigarette and liquor sales, child custody, and taxation—in most of which conflicts the tribes have won out. There have been growing efforts by tribes and state and local governments to negotiate jurisdictional agreements.

During part of the present self-determination era, Theodore Taylor served as a BIA official, and Vine Deloria, Jr., a Standing Rock Sioux, was for three years (1964-1967) the executive director of the National Congress of American Indians. Taylor is now a lecturer and consultant. Deloria, who is most noted for his first book on Indian affairs, *Custer Died for Your Sins*, and his co-author, Clifford Lytle, are professors of political science at the University of Arizona.

The Deloria-Lytle book, which is subtitled *The Past and Future of American Indian Sovereignty*, leaves out too much of the present. Taylor's book, one in a Westview series on federal agencies, is better on this score. The Deloria-Lytle book does make some suggestions for the future—for example, that tribal councils be doubled or tripled in size as a step toward making the three branches of tribal governments more separate and independent from each other. But the Arizonians' book is hard to follow—and seems a little too romantic—when it discusses the future of the conflict between what is called the "realism" of tribal officials and the "idealism" of ill-defined "tribal peoples" or "traditional" members of tribes, in respect to tribal decision making.

There is still a need for a general book on American Indians and government that focuses on the present-day relationship of Native Americans individually to the federal government and on the politics, powers, and governments of Indian tribes in the federal system. Until one appears, these two books are welcome and valuable additions to the literature.

FRED R. HARRIS

University of New Mexico

The Politics of the American Civil Liberties Union. By William A. Donohue. (New Brunswick, N.J.: Transaction Books, 1985. Pp. xix + 390. $29.95, cloth; $14.95, paper.)

William A. Donohue is a traditional conservative, influenced by writers such as Sidney Hook, Robert Nisbet, and Samuel Huntington. He affirms a commitment to the Bill of Rights, yet as a conservative, he strongly believes that at some point liberty must be balanced with social order if individual freedom is to be realized. As a civil libertarian, Donohue supports the idea of a citizens' litigation and lobbying group to protect the Bill of Rights, but he argues that the American Civil Liberties Union (ACLU) has put much more emphasis on working for the latest political liberal causes than on advancing the cause of civil liberties. Since the late 1960s, the ACLU has put great emphasis on expansive interpretations of civil rights legislation, backing the egalitarian platforms of black, women's, and other social movements of the recent past. Civil rights is not civil liberties, Donohue argues; the ACLU should concentrate on defending the first ten amendments and not put so much emphasis on extending the egalitarian implications of the Fourteenth Amendment.

From its founding in 1920 until 1940, the ACLU, Donovan maintains, was essentially a left-liberal fragment of the labor movement, concentrating on defending the rights of labor unions, while only incidentally expanding the civil liberties of the citizenry as a whole. The Molotov-Ribbentrop Pact put an end to the ACLU's fellow-traveling sympathies to communism, and the organization entered a relatively centrist twenty year period in which it did less for the unions and more for freedom of speech, and was particularly careful to avoid being branded as un-American by adopting a solicitous attitude to the anti-communist and national security concerns of those times. In 1966-67, however, the ACLU once again moved to the left, and since then has incorporated many of the egalitarian concerns of left-liberals into its interpretation of what constitutes civil liberty. In addition, the ACLU developed, in Donohue's eyes, extremist interpretations of freedom of speech (e.g., the Nazi march in Skokie controversy) and of the rights of criminals. The left-liberal platform and extremist individualism are likely to discredit the whole cause of civil liberties with the general public, Donohue believes, and accordingly the ACLU may now be doing the Bill of Rights more harm than good.

Most of the book's substance is directed at demonstrating that various ACLU positions, organized by time and category of action, are actually reflective more of the political-liberal notions of the day, rather than of a nonpartisan defense of civil liberties. Donohue has apparently spent two years or more pouring over the entire corpus of the ACLU's own written record, and his charges of liberal cooptation of the ACLU are copiously documented. In this reviewer's opinion, Donohue is basically successful in this major (although limited) goal of demonstrating the importance of liberal ideology within the ACLU. He is not entirely successful, however, due to his mode of presentation. Most of the book consists of a sort of adversarial brief in which Donohue presents his case that various ACLU positions demonstrate an adherence to liberalism, rather than to civil libertarianism. Because the ACLU itself, and several other scholarly authors have already argued the opposite case for nonpartisanship, Donohue sees no particular need to explain why the ACLU sometimes devotes considerable effort to oppose liberal causes, such as its opposition to the post-Watergate federal campaign finance laws. Nor does he explore the rather obvious question of whether the ACLU has recently adopted its own independent interpretation of liberalism, an ideology suitable to its organizational needs and which stresses an extreme individualism in civil liberties, even as many liberals have become more centrist in respect to affirmative action, the rights of criminals, school busing, and pornography during the relatively conservative 1980s. The presentation of Donohue's case an an adversarial brief, not considering at length opposite argumentation, leads the reader to be reluctant to accept the full range of Donohue's claims.

If the ACLU organization was compared to a mixed drink, then, the ACLU itself argues that its potion is nine parts civil liberties to one part liberalism. Donohue, on the other hand, argues that the ACLU mixes about three parts liberalism to one part civil liberties. After perusing Donohue's book, readers are likely to conclude that the ACLU is a half-and-half cocktail.

Most of the book is well-written and interesting. The reader should not be put off by a relatively boring section near the beginning (pp. 56-103) in which the author rehashes familiar arguments about affirmative action programs and other egalitarian goals of recent liberal social movements. After page 123, the book is often fascinating, particularly when Donohue recounts several incidents that illustrate the all too human foibles of various ACLU leaders. Especially interesting are: the narration of the ACLU's attempt to censor the content of an article about itself in the *American Mercury* in 1938; the purge of communists from ACLU leadership positions in 1940; the "palsy-walsy" relationships between ACLU officials and J. Edgar Hoover in the 1950s; and the details of the controversy over the 1977 march in Skokie.

Donohue was not trained in interest group theory, and thus the book will be a disappointment to research specialists in that subject. For the most part the book does not treat questions put forth by Michels, Truman, Olson, Salisbury, etc. There are no references to these theorists or to others of their ilk. James Q. Wilson's studies of crime are cited, but not his research on interest groups. Donohue's book focuses almost exclusively on ideology.

ANDREW S. MCFARLAND

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The Voting Rights Act: Consequences and Implications. Edited by Lorn S. Foster. (New York: Praeger Publishers, 1985. Pp. vi + 208. $32.95.)

Although they are of uneven quality, Lorn Foster has successfully assembled a collection of essays on the Voting Rights Act of 1965 (VRA) which is required reading for scholars who engage in serious research in the areas of black politics and public policy. The essays seek to assess how and in what ways the VRA has produced changes in the American polity. Some of the essays also provide discussions of the implications of these changes. The essays are presented under five headings, the first of which is the "Overview." Congressman Don

Edwards is the sole contributor. Edwards briefly summarizes the discriminatory actions and practices employed against blacks following the Civil War, the continued voting discrimination against blacks in the 1960s and 1970s, and the controversy that transpired over the extension of VRA in 1982. He argues that there is an imminent and present need for legislation that prohibits racial discrimination in the exercise of the franchise.

The second major heading, "Judicial Impact," begins with an essay by Richard Engstrom, focusing on the development of legal standards to govern vote dilution litigation (e.g., gerrymandering and submergence). Following a lucid analysis of the cases, Engstrom cogently argues that the Supreme Court has pursued a course that limits the activity of the judiciary and the thrust of the federal government under the VRA. That is, the court's requirement of proof of discriminatory purpose provides it (the court) an "escape hatch" from confronting the problems inherent in vote dilution cases. Engstrom argues not for proportional representation, but simply for "neutral treatment." The emphasis of Katherine Butler's essay is the comparison of the Supreme Court's definition of the right to vote under Sections 5 and 2 of the VRA. The comparison is not as sharp as it could be. She argues that the Supreme Court "does not see dilution as an infringement upon the right to vote, at least not the right that is fundamental" (p. 47). Implicitly, Butler suggests that the focus of the VRA should be to provide the open competition of blacks "in the pluralist bazaar of politics."

The third major heading, "Legislative Impact," contains three essays. The first, and perhaps the most innovative in analysis, is Mack Jones's contribution. Using Edelman's *The Symbolic Uses of Politics* as his theoretical basis, Jones argues that the VRA and its implementation constitute intervention strategy, which means "that when a public policy issue involves a confrontation between dominant and subordinant elements, or elites and nonelites, the official action taken by the state will often appear to be differential toward the subordinate element or the nonelites, but the actual implementation may favor the elites" (p. 64). Jones contends that his findings more than adequately support this thesis. Because the VRA constitutes an intervention strategy, Jones argues that the impact of the VRA has been more symbolic than substantive. Lorn Foster's contribution focuses upon the usage of political symbols by both the proponents and opponents of the extension of the VRA in 1982. In the final essay in section three, Mark Stern examines (1) the extent to which the voting of white Southern House members on the 1982 extension is influenced by the proportion of blacks in the district, and (2) variations in senatorial support for legislation that advances the rights of blacks. Stern's effort reveals some various interesting and provocative findings.

The fourth section, "Administrative Enforcement," contains two well-written essays. In the first, Dale Krane evaluates the implementation of Section 5 of the VRA in light of several myths (e.g., local officials must beg federal officials for clearances, and blacks have unlimited access to the Department of Justice), and from the perspective of local officials. Krane argues that negotiation, advisement, and informal bargaining are the major means of implementation. Hunter's contribution focuses on the VRA and the states' rules governing the right of voters in need of assistance in order to vote.

In the final section, "Electoral Response," Charles Bullock, in a longitudinal study of Atlanta, Georgia, examines the cohesiveness of blacks and whites in support of members of their race in elections. Bullock finds that blacks had greater cohesiveness than whites. Bullock also indicates the circumstances under which black and white cohesiveness decreases or increases. He contends that the slightly greater cohesiveness among blacks may be attributed to the historical discriminatory exclusion of blacks from the political process.

On the whole, the message of the collection of essays is that while the VRA—the most comprehensive and forceful piece of legislation enacted in the 1960s—has made significant inroads in reducing voting discrimination, such efforts must be continued.

MICHAEL W. COMBS

Louisiana State University

The Dynamics of Foreign Policymaking: The President, the Congress, and the Panama Canal Treaties. By William L. Furlong and Margaret E. Scranton. (Boulder, Colo.: Westview Press, 1984. Pp. xxii + 263. $19.00, paper.)

This case study of the negotiation, ratification, and implementation to date of the controversial 1977 Panama Canal Treaties is presented within an analytical framework designed to facilitate broader comprehension of the dynamics of American foreign policy making. Utilizing source material drawn from interviews, public records, memoirs, journalistic accounts, and scholarship, Furlong and Scranton employ and integrate three perspectives: organizational—embracing the separation of powers, bureaucratic, and cross-cutting coalition approaches; bargaining; and ecological.

From the alternative vantage points, the pro-treaty authors survey the history of U.S.-Panamanian relations, explanations for the fundamental reorientation of Canal policy embodied in the treaties, and the activities of the occupants of key institutional roles in the treaty process: presidential, bureaucratic, and congressional. Ironically, they undermine the general applicability of their case by demonstrating that from the outset, Panama Canal policy making has deviated from conventional foreign policy making expectations and patterns. The original 1903 treaty's provision for American sovereignty over the Canal transferred it into the domestic arena. Procedurally, congressional participation has always been abnormally high, as has the level of interest and emotional involvement from pressure groups and the general public.

Decades of Panamanian demands for changing the terms of the relationship foundered on the dogmatic U.S. insistence upon maintaining effective control over the Canal. The authors pinpoint the first sign of official willingness to shift U.S. policy toward cooperation and partnership in the 1973 Nixon administration, in response to an internationalization strategy undertaken by Panamanian leaders, whereby the U.N. Security Council met in Panama and discussed the conflict. They place this catalyst in ecological context, amid easing East-West tensions and anti-interventionist sentiments that were opening a window of opportunity for bargaining.

Administration policy makers elevated the priority assigned to issue resolution. A new chief negotiator was designated in 1973, and the secretary of state concluded an agreement on principles with his Panamanian counterpart in 1974. Within the executive branch, the Departments of State and Defense nominally reconciled long-standing policy differences in 1975, and three separate negotiating rounds with the Panamanians occurred in the periods 1973-75, 1975-76, and in 1977. Along the way, the Nixon administration passed into history, giving way to the Ford, and then the Carter presidencies. A systematic comparison of the three administrations with regard to seven expected executive activities, opportunities for action, actions taken, and impact results in more favorable evaluations for Nixon and Carter, with Ford's lower rating attributed to the politicization of the Canal issue during the 1976 presidential campaign.

Turning to Capitol Hill, the Senate's enthusiastic exercise of constitutional prerogatives, the surprisingly assertive role of the House in foreign affairs policy making, the House-Senate rivalry, and the drawbacks of congressional involvement in treaty making are major themes developed. The authors argue that Senate deliberations "strengthened, clarified, and improved" the treaties, generating a razor-thin margin of consent. In contrast, they disparagingly characterize the actions of the House on subsequent implementation legislation as engendering "antagonism, controversy, and misunderstandings" (p. 163). Again placing events in context, the window of opportunity was closing dramatically by the late 1970s, with heightened concern over communist aggression and revolutionary violence in Latin America. Fortuitous timing thus emerges as the critical factor accounting for the triumph of the treaty proponents in changing policy.

The authors perceive in this treaty battle within and between the executive branch and Congress a clash of world views: liberal versus conservative internationalism; or, more simply, cooperation versus confrontation. Their broader conclusion is that this enduring division precludes attainment of the consensus they hold essential for efficacious foreign policy making in a separation of powers system. Supplementary reference material includes a chronology, maps, texts of the 1974 Kissinger-Tack Agreement and the 1977 Treaties, and a selected bibliography.

HAROLD F. BASS, JR.

Ouachita Baptist University

The Fraud Control Game: State Responses to Fraud and Abuse in AFDC and Medicaid Programs. By John A. Gardiner and Theodore R. Lyman. (Bloomington: Indiana University Press, 1984. Pp. xvi + 266. $24.95.)

John Gardiner and Theodore Lyman have written a well balanced study of states' efforts to control fraud in AFDC and Medicaid programs. The issue is an interesting one from normative, methodological, and organizational perspectives. From a normative perspective, one may debate the significance of an estimated $391 million in overpayments in the $12.5 billion AFDC program or an estimated $668 million in improper payments in the $22.8 billion Medicaid program. A methodologist might explore techniques for accurately measuring fraud and abuse in these programs. Not only is there a serious problem of detection of fraud and abuse, but the programs are complex which leads to difficulty in determining when an over- or underpayment has occurred. Even if one ignores the complexity of the programs, the areas of uncertainty remain. A physician may charge for an "extended," rather than a "limited" consultation, but the difference between the two is not at all clear. Professors Gardiner and Lyman touch upon these normative and methodological perspectives, but it is as a contribution to organizational theory that their study excels.

Borrowing from Norton Long's work on the ecology of games, Professors Gardiner and Lyman note that within each game actors use their resources in following the rules of the game in order to win desired outcomes. In the area of welfare policy, several games affect the character of fraud control policies. Fraud control is affected by the activities of welfare rights groups, poverty lawyers, social work schools, and private charities. All of these groups are dealing with the interests of recipients. Providers of health care, health care associations, insurance companies, and health departments are concerned with health policy issues and thus play a different game with different policies and goals than welfare-recipient interest groups. Criminal justice agencies are concerned with the processing of fraud cases, a different game entirely from the one of welfare recipients or health care providers. Numerous other games are played in the welfare policy area, and fraud control policies will be affected by the varying mixes of competing interests and goals affecting welfare programs.

As a result, fraud control policies will vary from state to state. Absent a major welfare scandal or other high visibility welfare fraud, control interests would tend to be minimal in relation to other interests pursued by crucial players. Thus, prosecutors would have little interest in minor fraud prosecutions since violent crime prosecutions get their primary attention. Additionally, a successful criminal prosecution of minor fraud offers little payoff. It wastes prosecutors' resources, angers low income voters, has little positive political visibility, and could cost local government more for family upkeep if a welfare mother is jailed. Similarly, welfare officials might tolerate provider fraud in the Medicaid program since elimination of that provider could limit health care for medicaid recipients.

In appendices to the book, Professors Gardiner and Lyman show in detail the variation in fraud control efforts in three states, and there, and in the text, they suggest how some structural changes could enhance fraud control efforts.

In sum, the book is an excellent treatment of how competing interests and goals in a policy area limit effectiveness in achieving one policy goal.

ANTHONY CHAMPAGNE

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Dallas

The New American Dilemma: Liberal Democracy and School Integration. By Jennifer L. Hochschild. (New Haven and London: Yale University Press, 1984. Pp. xvi + 263. $27.00, cloth; $8.95, paper.)

Hochschild's book is compelling in several respects. Her work is neither another mundane program evaluation documenting yet another policy failure, nor an illustration of a more sophisticated statistical technique yielding unremarkable findings. Her effort is a refreshing addition to the literature of policy analysis—an artful integration of normative questions and empirical theory aimed at the specification of practical policy recommendations. *The New American Dilemma* illustrates her argument: "We can use the lessons of policy analysis and empirical theory to address our normative problems" (p. xiii).

Through an extensive review of the literature on school desegregation, she clarifies the reasons for program failures in many American schools. Specifically, she concludes that incremental strategies and popular control have generally frustrated the achievement of full-scale school integration. This conclusion clearly underscores the collective intention of halting progress of desegregation. In contrast, many other policy analysts have offered explanations focusing on factors that suggest an absence of community responsibility: white flight, anti-busing sentiments, preferences for educational quality in private schools, or black demands for neighborhood control of schools.

Having diagnosed the problem in typical strategies used for reaching the goal of integration, she is able to generate policy recommendations based on previous school district experiences where success stories involve rapid, massive adjustments in school systems—adjustments often facilitated by unelected federal judges or bureaucrats. As a result, she underscores the utility of policy analysis for determining which strategies are more effective for achieving program goals.

The broad-based design of her policy analysis allows Hochschild to add to an understanding of incrementalism. In this case, her conclusions reinforce the fruitfulness of the concept for accurately describing routine decision making. Additionally, there is evidence from her analysis that some popular control over school district policies actually exists. On the other hand, the argument that incremental strategies will gradually lead to major social change is not confirmed in Hochschild's interpretation of the evidence. Furthermore, she directly confronts the issue of majority decision making versus minority rights; in other words, she finds little support for a belief that majorities in school districts (often white majorities) will ultimately embrace meaningful—rather than minimal—school integration.

These two major conclusions lead America to its new dilemma—the question of how to achieve school desegregation, or racial justice for that matter, without relying on the normal procedures: incrementalist strategies and popular control. Hochschild sees the best choice in positive leadership by political elites. On the other hand, she is not without fear that significant change is unlikely: "The big danger is that we will *neither* desegregate successfully *nor* pursue other ways to fulfill the promise of *Brown*. In that case, minorities and Anglos alike will suffer the consequences of our political, theoretical, and moral failings" (p. xiv).

The potential criticisms of Hochschild's type of policy analysis are both apparent and predictable—her work is a nonquantitative secondary analysis, her conclusions are overly subjective, and her policy recommendations are both unrealistic and undemocratic. On the other hand, I wonder where the critics would find a better policy analysis of desegregation—a better policy analysis, or a better use for policy analysis for that matter. Hochschild's book may be an important vehicle for increased interest in explicitly including normative questions in policy analysis. She provides a blueprint for doing so without sacrificing either existing empirical theory or real-world politics.

JILL CLARK

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The MX Decision: A New Direction in U.S. Weapons Procurement Policy? By Lauren H. Holland and Robert A. Hoover. (Boulder, Colo.: Westview Press, 1985. Pp. xiv + 289. $22.50.)

The Making of International Agreements: Congress Confronts the Executive. By Loch K. Johnson. (New York: New York University Press, 1984. Pp. xx + 206. $30.00.)

Legislating Foreign Policy. Edited by Hoyt Purvis and Steven J. Baker. (Boulder, Colo.: Westview Press, 1984. Pp. x + 229. $22.00.)

Much has been written in the last decade about the changing relationship between Congress and the executive. The literature has argued, almost without exception, that since the early 1970s Congress has been more willing to challenge the executive on a wide range of issues concerning foreign and defense policy than was the case in the earlier post-World War II period. These three books are additions to that literature and each supports this proposition.

While Holland and Hoover devote considerable attention to congressional-executive relations, their book has the broader purpose of testing to see if the complex procurement of the MX missile is consistent with the bureaucratic-politics model of decision-making. In their descriptive chapters the authors discuss a wide range of issues including the window of vulnerability argument and the foreign policy implications of MX. They also provide a detailed review of the MX's history from its inception in 1973 through the Reagan administration's attempts to find an acceptable basing mode. Their chapter, "MXing It Up in the Great Basin," which discusses the political struggle over basing the MX in Utah and Nevada, tends to place too much emphasis on that aspect of the decision, but this probably reflects that both authors are from universities in Utah. As a case study, this book joins the growing and rather substantial literature on the MX and is a worthy addition to it. The troublesome portion of the book is the authors' effort to test the bureaucratic-politics model.

In developing their version of the bureaucratic-politics model, the authors conclude that, among other failings, this approach does not give sufficient attention to the role of Congress and the public. What they overlook is that this model generally is used to examine behavior in the executive branch and, consequently, is not intended to include other aspects of decision making. While they concede that the literature is "ill-defined" (p. 17), they attribute to it more consensus than exists in order to support their criticisms. Also, there is confusion about the role of the president. At times they assign him the role of being a central figure that dominates the process and at other times give him no more than a persuasive role (p. 14). Based on their criticisms of the bureaucratic conflict model, the authors come close to advocating Graham Allison's rational-policy model.

Another problem with their analysis is that they argue that bureaucratic-politics analysts are wrong in maintaining that "strategic values are of secondary significance to bureaucratic participants in procurement decision making," and they believe that "organizational disputes over weapon procurement are partially explained by *real* differences over strategic matters" (p. 29). This interpretation overlooks the literature on bureaucratic politics which recognizes that there are indeed real, not secondary, differences over policy, and that this is the result of each bureaucratic unit seeing the situation from its own perspective. All is to be forgiven, however, when in the last chapter the authors admit that "In setting ourselves the task of testing the validity of the bureaucratic politics perspective . . . we have of course erected a straw man" (p. 245). The reader feels betrayed and must wonder why so much effort went into analysis of the bureaucratic-politics model if it was only to serve as a straw man. They conclude that of 21 propositions they were testing concerning the bureaucratic politics model, 13 were confirmed by their case study, 3 partially confirmed, and 5 rejected. The approach was, apparently, confirmed.

Johnson's book is divided into two distinct parts, with the first making the greater contribution. After classifying international agreements as to subject matter, Johnson also breaks them down by form and administration. The period covered is 1946-1972. It is unfortunate that his statistics do not go beyond 1972. Instead, his data end just when a more assertive Congress is developing. As to the form of international agreements, instead of the usual comparison of only executive agreements and treaties, Johnson includes statutory agreements—a category of agreements that assures congressional action for implementation. Over the five administrations he covers—Truman through part of Nixon's term—86.7% of all international agreements are statutory agreements and 6% are treaties, both of which require congressional review. Only the remaining 7.4% are executive agreements. From this Johnson concludes that Congress has a greater *procedural* involvement in the making of agreements "than conventional wisdom suggests . . . however one cannot conclude necessarily that the Congress plays a large *substantive* role" (p. 26). In a later chapter, "The Hidden Side of Agreement-Making," Johnson makes clear that the executive has made a number of important military executive agreements that have bypassed Congress. Johnson also analyzes the countries with which the United States has made agreements and finds that the U.S. has "tended to enter into agreements chiefly with democratic nations" (p. 52), although the more recent the administration, the higher the percentage of agreements with authoritarian regimes.

In the second part of his book, Johnson reviews the fight over the Bricker Amendment and the Case-Zablocki Act, and then discusses the resurgence of Congress in the making of foreign policy. The information and analysis here is not new, but the discussion is reflective of solid research. His conclusions about a more assertive Congress are, because of the limitations of his data in part one, limited to the arguments of part two.

In editing *Legislating Foreign Policy*, Purvis and Baker are less ambitious in their purpose than the authors of the other two books. Most of the chapters are legislative histories of United States relations with another country, including the congressional role in policy toward Turkey, the Panama Canal treaties, arms sales to country X, the sale of nuclear fuel to India, and the SALT treaties before the Senate. The case studies are well-written, but are researched mainly from government documents and only occasionally mention other works. Purvis, who wrote the introduction, served for many years as an aide to Senator Fulbright, and he and Baker (who wrote the conclusions as well as the chapters evaluating Congress, the nuclear fuel case, and arms control) served as advisors to Majority Leader Byrd. Their experiences and access to these central figures in the Senate is reflected in their commitment to a strong role for the Senate in foreign policy, and they give Congress high scores in its foreign policy activities. In their rating system, Congress is given 13 positive ratings, and 6 qualified positive ratings out of 25 categories. The book says little about the capabilities of Congress to make and review foreign policy, however.

No one of these books is a major contribution to the literature on congressional-executive relations, but, with the exception of the discussion of the bureaucratic-politics approach in Holland and Hoover, each has something to offer.

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Party Identification, Political Behavior and the American Electorate. By Sheldon Kamieniecki. (Westport: Greenwood Press, 1985. Pp. xiv + 288. $35.00.)

Despite its title, Sheldon Kamieniecki's book is narrowly focused on the conceptualization and measurement of an individual's attachments, feelings, and orientations toward parties, partisanship and independence. Ever since John Petrocik's discovery of "intransitivities" in the seven-point party identification scale, questions have been raised over what this scale, in fact, measures. These questions in turn prompted further questions about the conceptualization of an individual's partisan attachments. These efforts led some researchers to hypothesize that an individual's party attachment is multidimensional in character, a possibility that led to the development of new instruments to tap these dimensions and their inclusion in the 1980 National Election Study. Sheldon Kamieniecki uses these new partisan measures to confirm and refine Herbert Weisberg's multidimensional conceptualization of party attachments. Kamieniecki also explores which measure best taps each of these dimensions.

After first reviewing the various controversies surrounding party identification, Kamieniecki replicates Weisberg's factor analysis of these same variables. With only several minor differences he finds the same four-factor solution. However, an attempt to confirm Weisberg's identification of one factor as a party-system support factor failed since this factor was found to be unrelated to Dennis-like party-system support variables. Since the content of this factor could not be identified, Kamieniecki deleted those variables which had only high loadings on this factor and redid the analysis. Three clean factors emerged: (1) a partisan direction factor defined by high loadings for the seven-point party identification scale, the party support and party closeness scales, and a party difference scale derived from subtracting the Democratic party feeling thermometer from the Republican party feeling thermometer; (2) a partisan strength factor defined by the folded party identification, party support and party closeness scales; and (3) an independence factor defined by the strength of the independence scale and the independents feeling thermometer.

To confirm further this three-factor solution, and the utility and interchangeability of the various measures defining each dimension, Kamieniecki, following the logic of criterion-validity analysis, examines the simple correlations between each of the above measures of partisan direction, partisan strength and independence, and a host of criterion variables. The criterion variables examined included demographic variables, parents' party identification, psychological-political involvement, media attentiveness, party system support, political trust and efficacy, attitudes toward groups and issues, ideology, candidate evaluations, participation, vote choice, and volatility of vote choice. For each category of criterion variables Kamieniecki provides a brief summary of previous findings regarding their relationship with party identification, and notes whether the pattern of correlations supports a multidimensional characterization of party attachments, and whether there are statistically significant differences between the correlations for each criterion variable and the partisan measures defining each factor. This analysis comprises about 75 percent of the entire book. The principal conclusions drawn from this mass of correlation coefficients are: (1) party attachments are multidimensional, and in particular partisan strength and independence are separate dimensions of an individual's partisan attachments; (2) the party difference measure is the best measure of partisan direction since it has the strongest relationship with most criterion variables including vote choice; and (3) for the same reason the folded party identification scale and the strength of independence scale are the best single measures of partisan strength and independence, respectively.

Kamieniecki's research is an exhaustive analysis of the meaning and validity of these measures of partisan attachments. As such, it should be emulated in other areas since all too often we use measures whose meaning and validity have not been determined. On the other hand, his analysis does not offer much that is new, and merely confirms what was already known or strongly suspected about these measures. Even at its own level one might want to object to some of his conclusions. For example, the best measure of each dimension may not be any of the individual measures but the factor scores themselves. The failure to examine this possibility is a significant limitation of the study. Furthermore, the difference between the correlations for a particular criterion variable and the partisan measures tapping a given dimension rarely exceeded .1—a rather frail base on which to select the best measure of a dimension. Finally, the superiority of the party difference measure may be due to the contamination of the party feeling thermometers by short term forces and the fact that its construction is dependent on the traditional party identification scale to distinguish between those who rate the parties equally on the thermometers.

While this book will be of some interest to party identification specialists, its focus and findings are sufficiently limited that its appeal to a broader audience will also be rather limited.

WILLIAM CLAGGETT

Florida State University

Political Equality in a Democratic Society: Women in the United States. By Mary Lou Kendrigan. (Westport, Conn. and London: Greenwood Press, 1984. Pp. ix + 149. $27.50.)

This study in a "Contributions in Women's Studies" series is a feminist essay, 89 pages in text, critically evaluating equality as an essential, irreducible element on the feminist agenda. In a systematic, scholarly and readable fashion, it performs a service to committed feminists who must come to understand fully the substantive demands of their politics, and to non- or uncommitted others who want simply to understand the nature of the feminist challenge to our political theory and institutions. Ms. Kendrigan dispels the gloomy notion that in the confrontation with traditional democratic theory, feminism is too theoretically dissipated to contribute much. She clears the air about the concept of equality by determining its variable meanings and their policy consequences, and initiates some systematic thinking, long overdue, which might have prevented the more egregious policy fiascoes undertaken in the name of equality. The work is a model of feminist methodology, combining theoretical and empirical analyses of equality, on one level, with programmatic advocacy of an equality of rights, on another.

A pragmatic feminist, Ms. Kendrigan adopts a nonabsolutist approach best labeled "Taking Equality Seriously, and Moderately," in which she shows how the cumulative social, political, and economic inequalities found in American society reinforce each other to keep women down. In the women's rights tradition, her remedy is political equality based upon the effective representation of women's interests, through a politics wherein the sex of the participants is irrelevant to power. Her political activism is strewn with bottom-line advice and modest expectations, e.g., a concluding caveat that the best pro-feminist policy attainable may be only marginally successful.

The author's argument is a familiar one. She extensively documents existing political, social, and economic inequalities: a cycle of political oppression, restrictive cultural mandates, and occupational segregation that destroys women's persons and potential. Anticipating the objections of those optimistic about the considerable social change recently, she enlists the sociological concepts of stratification and institutional sexism to explain how system persistence can neutralize if not negate surging social forces. Change-induced differences in society need not represent the increased equalities for which feminists hope.

The analysis of equality represents the strongest part of the essay. Ms. Kendrigan distills the concept into three "essences." One is equal consideration of interest (or, no distinctions except upon proper justification), whose inadequacies are a "negative egalitarianism" and a blindly benign attitude to the status quo. Another is equality of opportunity (or, equal rights and equal rewards—fair rules), likewise inadequate because it works best only in the best case: one of initially equivalent circumstances. The real-life question of equal treatment in the face of differences disarms it. Finally, there is equality of results, the recommended choice for feminists. It seeks, apparently, an equality sufficient to allow equitable participation of both sexes in public life. By stressing similarities and treating differences differently when the common/particular good requires it can decrease inequalities and increase equalities. The ultimate theoretical justification is an irreducible democratic faith in the community of humanity; the ultimate empirical justification, the adjustment of feminist political behavior to real politics by a focus on policy outcomes.

The conclusion specifies the political activities requisite to eventual political equality: ideological, self-interested voting by women; electing feminists who produce at least some significant policy changes; and sustaining the women's movement as the organizational basis of the entire endeavor—in effect, the political equivalent of a vertical merger. The author has successfully presented a feminist critique of democratic theory from the viewpoint of the powerless and has clarified a vital segment of feminist politics.

A critique of equality of results deserves another essay. More manageable is a note on the weakness of the post-analytical discussion on political strategy. Substantive vs. procedural, process vs. substance are familiar and well-taken distinctions. Here they lead to disappointingly familiar conclusions: process is important but policy content and results are ultimate proof of quality. For all its timelessness, advice that feminists must be vigilant and must continuously monitor and evaluate policy is anticlimactic. It adds nothing to either the decade or more of labors nearly preempted by the policy-analysis subfield of the discipline, or to the Eastonian systems model introduced over 20 years ago. The essay still deserves a wide reading, certainly wider than its prohibitive price will allow.

MARY R. MATTINGLY

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American Arms Supermarket. By Michael T. Klare. (Austin: University of Texas Press, 1984. Pp. xii + 250. $10.95, paper.)

With all the attention paid to arms racing and conflict resolving by scholars who investigate the causes of international competition and war occurrence, the subject of arms transfers is often treated as ancillary to national security analyses whose focus concentrates on nuclear armaments, superpower rivalries, or crisis management. To be sure, arms races, conflict resolution, and war occurrences are in part based on the possession of weapons and their accessories (suppliers, spare parts, training and maintenance, etc.) obtained by national development or international access, but the literature on arms transfer is only now growing in quality and quantity to bring this critical aspect of world politics and economics into perspective. Readers familiar with Andrew J. Pierre and Anthony Sampson, for example, will welcome Michael T. Klare into this circle.

In *American Arms Supermarket*, Michael T. Klare has produced a tour de force. Beginning with a set of empirical questions and employing a cluster of factors to guide the discussion, he presents a comprehensive history of U.S. foreign policy from the early 1950s to the mid-1980s wherein arms transfers are seen as an increasingly important instrument of American national security with respect to Europe and the Third World (especially the Middle East and Latin America). Klare analyzes the historical, political, economic, and psychological assumptions behind this country's arms policies toward allies and potential allies, and, more importantly, why this instrument does more harm to U.S. interests in the long term than good.

There are three models either explicit or implicit in Klare's study: the bureaucratic politics model, the dependency model, and the interdependency model. The bureaucratic politics model helps us sort out the many political actors involved in American arms policy making (State, Defense, National Security Council, Central Intelligence Agency, Arms Control & Disarmament Agency, Commerce, and Treasury, to name the well-known but not all the important ones such as International Security Affairs, Defense Security Assistance Agency, Military Assistance Advisory Groups in the Defense Department, or Politico-Military Affairs in State, etc.), first in the executive branch, and then in presidential-congressional relations over arms transfers. The dependency model is used to show how the United States in the 1950s and early 1960s once used arms transfers to influence allies and clients in ways generally favorable to American security needs. Today, on the other hand, this policy seems to make Washington dependent on its allies and clients, being careful with these arms transfers not to set off conflagrations in volatile regions like the Middle East or Latin America. The interdependency model assesses two phenomena involving arms transfers: coproduction along with technology infusions from this nation to other nations; and control by first having a new policy agenda, and then working closely with other arms powers to slow down arms transfers that clearly contribute to regional instability or violence. In all, this book is a benchmark of research, analysis, and utility that will be cited in the literature of national security wherever arms transfers are the subject of controversy.

JOSEPH RICHARD GOLDMAN

University of Minnesota

Television's Window on the World. By James F. Larson. (Norwood, N.J.: Ablex Publishing Corp., 1984. Pp. x + 195. $27.50, cloth; $17.95, paper.)

With its genesis the author's 1978 Ph.D. thesis, Larson presents a painstaking study of a random sample of 13% (some 1,000 broadcasts) of the coverage of international affairs on the weeknight evening news shows of the three U.S. television networks from 1972 through 1981. He finds that 40% of the evening news—seven out of 17 stories on an average night—falls into the international affairs category (defined as a "news story that mentions a country other than the United States, regardless of its thematic content or dateline" (p. 36). Of these international stories, 42% consisted of the anchorman reading an item, usually from a wire service, with a still picture, map, diagram, etc. in the background. Another 26% were domestic video reports originating live or taped from the White House, State Department, Congress, the Pentagon, or some simulation thereof. Only 32% originated from outside the U.S.; they were normally prepared by network correspondents and transmitted by satellite.

Digging deeper into his data, Larson shows "that the overall pattern of network news attention is highly skewed toward a small number of nations" (p. 55). Right at the top, in fact, with some 60%, is the U.S. itself, followed by the USSR (17%), Israel (14%), and then Britain, South Vietnam, Iran, Egypt, North Vietnam (after 1976 all references to Vietnam were coded North Vietnam), France, and China, with between 10 and 5% each. No other state had more than 5%. The nations appearing most often "were involved in wars or major conflicts" (p. 57), and/or were major world powers.

Most coverage originating overseas comes from Western Europe and the Middle East. With the exception of major crises, or when events there involve the U.S. or other developed states, "the U.S. television networks have paid minimal attention to Latin America, Africa, South Asia, Southeast Asia following the Vietnam war, and Eastern Europe if the USSR is excluded" (p. 62). And quite often coverage of the Third World, especially Africa, occurs only when there is a crisis (defined, p. 69, as unrest and dissent; war, terrorism, and violent crime; coups and assassinations; and disasters). Seeking to explain these coverage patterns, Larson discusses the increasing use of relatively cheap satellite communication; the global deployment of network bureaus and correspondents; and the availability to the networks of wire service reports. Of these, network bureau location is the strongest predictor of coverage. Also mentioned, but not considered in any detail, are news values, access (North Korea does not welcome U.S. television reporters), and foreign censorship.

The penultimate chapter is a cursory and too brief attempt by Larson to explore the implications of his data for the foreign-policy process. It does, however, contain the provocative observation, worth pursuing, that "the lack of network news attention to issues of social change and development in the Third World gives U.S. policymakers broad leeway to ignore, minimize, or postpone consideration of such problems" (p. 143). Larson concludes by asking whether television can portray the urgent struggle for health, education and agriculture in developing countries and show the American public how it is involved in such problems.

The book's distinctiveness is that it is a longitudinal study that subsumes (on a sample basis) all coverage of international affairs. The author mines his data with diligence and care. He is able to confirm that the networks' evening news shows are less a window on the world than a shadowy glass. And he shows us the shape and size of the glass.

The work suffers from two major problems: inadequacies of content analysis, and circumspection. First the content: the subject is television news, but there is no analysis of the visuals—of what viewers actually saw. Worse, there is no discussion of the stories' actual content, of their approach and perspective. We learn little about how the events in question are depicted. Only because of such limitations can Larson report that coverage does not differ from one network to another when in fact a recent book by Dan Nimmo and James Combs (*Nightly Horrors*, University of Tennessee Press, 1985), documents quite dramatic differences.

Perhaps because his data are so limited, Larson seems reluctant to venture beyond them. Thus the key question: "How completely and how well have [the three networks] . . . conveyed information about international affairs to the American public?" (p. 2) is posed, but goes essentially unanswered. Answering it would require the author to consider such topics as what should be covered, how the networks should deploy their resources, and what types of international affairs information would best serve the American public.

DAVID PALETZ

Duke University

Religion and the Presidential Election. By Paul J. Lopatto. (New York: Praeger Publishers, 1985. Pp. xi + 177. $32.95.)

This book makes a positive contribution to our understanding of the role religion plays in American electoral politics. It attacks the conventional wisdom among political scientists that the voting patterns of American religious groups are not particularly distinctive and attempts "to establish religion as a major explanatory variable in the study of American presidential voting behavior" (p. viii). While this book is not likely by itself to accomplish such a goal, it does demonstrate that when political scientists approach the relationship between religion and voting behavior with the same degree of sensitivity to subject matter and thoroughness in analysis as is generally applied to the study of other relationships, the persistence and importance of religion-based differences in presidential voting patterns become apparent.

Lopatto begins his book with a relatively brief discussion of the role of religion in American political history. This discussion serves as a context for the development of theoretical propositions that attempt to explain variation in the linkage between religion and presidential voting over time. Lopatto contends that such variation is due to both short-term factors (the particular selection of their presidential candidate by each party) and long-term factors (social, economic, and demographic changes at the mass level).

In the second chapter, Lopatto develops a denominational classification scheme differentiating between and among liberal, moderate, and conservative Protestants and Catholics on the basis of differences in religious beliefs. The remaining five chapters examine presidential voting patterns, and how attitudes toward the presidential candidates, attitudes toward major political issues, and demographic changes relate to such patterns within and across the four denominational categories through an analysis of data from the six national presidential election studies from 1960 through 1980.

Analysis revealed that religion plays a major role in affecting voting behavior by first affecting the development of attitudes toward many of the major issues of the day. While evidence linking religion with presidential voting was quite strong, the direction and impact of religion varied over time. In fact, based upon "normal vote" projections, the impact of religion appeared to be less in the 1976 and 1980 presidential elections than had been the case for the previous four presidential elections.

Does this apparent decline of the influence of religion in the 1976 and 1980 presidential elections suggest a weakening of the hold of religion in American electoral behavior? Not necessarily. Lopatto's analysis suggests that a great deal of variation in the magnitude of the linkage between religion and voting behavior is due to the changing nature of presidential nominees from one election to the next. Thus, the particular impact that religion had, for example, in the 1980 presidential election would likely have been different had Kennedy, rather than Carter, been the nominee of the Democratic party. Moreover, given the Reagan-Mondale pairing in 1984, it is likely that a replication of Lopatto's analysis for the 1984 election would reveal an increase in the religious impact upon presidential voting—with conservative Protestants voting more Republican and liberal Protestants more Democratic than might be expected on the basis of normal vote projections.

Unfortunately, the book gives no attention to the impact of differential rates of religious involvement for each of the denominational categories. Similarly, denominational classifications tell us little, if anything, about the impact of religion upon presidential voting for those religious groups that may be found within each of the four denominational categories (e.g., evangelicals v. charismatics). Thus, while religion may appear to have had minimal influence on voting patterns in 1980 based upon normal vote projections for the four denominational categories, its influence might have appeared greater had different religious groupings been analyzed. Nevertheless, this book represents an important contribution to the literature on the topic and deserves the attention of all those working in the area of American electoral behavior.

CORWIN SMIDT

Calvin College

The U.S. Anti-Apartheid Movement: Local Activism in Global Politics. By Janice Love. (New York: Praeger Publishers, 1985. Pp. xvii + 296. $38.95.)

The primary contribution of this study is able description along with systematic and judicious analysis of the roles and effectiveness in American politics as well as governmental and private decision making of domestic groups that favor termination of U.S. investment in and trade with the Republic of South Africa. While the author's sympathies clearly lie with these groups and their objectives, her interpretations are generally balanced and her judgments realistic. This book, moreover, provides considerable useful data not only on the composition, objectives, strategies, and activities of these diverse groups, but also on the contexts of U.S. investment, trade, and other linkages with South Africa which they hope to change. It also includes an excellent bibliography pertinent to the movement, its political milieux, and evaluations of the effectiveness of such social movements in general.

The book begins with an overview of the so-defined anti-apartheid movement, its international and domestic environments, its perceptions of South Africa and of changes in U.S. behavior apt to encourage desirable changes there, and its several objectives. A second chapter examines evolution of the sanctions debate both in the U.S. executive and legislative branches and in private life. The author then considers alternative criteria for evaluation and frameworks for analysis of the movement's impacts on policy making and on the American body politic more broadly. She thereafter applies her chosen goal-based criteria and evaluation design to the pertinent legislation which was enacted into law, or attempted, in two states: Connecticut and Michigan. These case studies included careful observation of the behavior of the respective legislatures and executives and interviews with most of the significant actors both in these state legislatures and governments and among the anti-apartheid supporters and their opposing interest groups.

By 1985, Michigan and Connecticut had enacted the strongest legislation regarding South Africa among state governments. However, the principal allies of the groups favoring state-enacted economic sanctions, the institutions to which they were applied, and the types of sanctions varied considerably between these two states. Even there, pro-sanction groups were unable to achieve much divestment or other sanctions applicable to private enterprises that had signed and were making apparently meaningful efforts to comply with the Sullivan Principles for American investment in South Africa. Consensus could be achieved on legislating only marginally beyond those principles, which the anti-apartheid movement regarded as little effective in bringing about significant change in South Africa. The movement succeeded in achieving passage of broader sanctions on investments by state pension and insurance plans by the Connecticut legislature, largely due to the business community's false impression that the legislation would not pass. But once passed, opposing commercial and other interest groups prevailed upon the Democratic governor to veto the legislation. A broadly representative task force appointed by the governor thereafter recommended a more limited bill, the essence of which did become law.

As the author notes, the relevant political and economic environments vary greatly from state to state. Short of considerably accelerated intensification of the black-white conflict in South Africa (which seemed more probable in August, 1985, than before), it seems doubtful that legislation much stronger or more inclusive than that in Michigan could become law in other states, or that any as effective as that in Michigan could be enacted in more than a few others. As the author implies, such states are apt to be governed by relatively liberal, Democratically controlled legislatures and governors, to be highly industrialized, and to have politically effective black elites, strong trade unions, world-class universities and other educational, research, and cultural institutions.

ALFRED O. HERO, JR.

Belle Chasse, La.

The Personal President: Power Invested, Promise Unfulfilled. By Theodore Lowi. (Ithaca, N.Y.: Cornell University Press, 1985. Pp. vi + 221. $19.95.)

Theodore Lowi, Professor of American Institutions at Cornell, has added another important book to an impressive collection. In the *Personal President: Power Invested, Promise Unfulfilled,* Lowi takes an historical and institutional approach in developing his summation of the presidency as too reliant on the slippery base of popular impressions. The "plebiscitary presidency" has emerged as political parties have atrophied and Congress has passed along a larger share of government's expanded obligations to the president. What is left is a personal presidency serving as the direct focus of popular demands. Every president, regardless of character, is therefore eventually deceitful in trying to raise expectations in order to win, to hold power, and to "deliver" on public promises. Inevitably expectations are elevated past the point of realization and failure is thereby guaranteed.

How did this state of affairs come about? Until the New Deal, the national government performed essentially patronage functions. However, during the transition period of the 1930s and 40s, the national government turned toward more regulatory and redistributive initiatives. This meant new governmental obligations, a longer reach for government, a rise of interest groups at the expense of parties, and a shift in program responsibilities from Congress to the president. It also meant a new theory of democracy with the presidency as its centerpiece. The legacy of Franklin D. Roosevelt and the decline of the mediating influence of parties and Congress left recent presidential candidates and incumbents personally responsive to the public as demands on big government multiplied.

While the investment of power in the president's hands democratized government, thereby increasing its concerns and initiatives, the costs have outweighed the benefits. The limits of solvency are quickly reached, for instance, if every program is seen as worthwhile in a large liberal state. Furthermore, the credibility of the presidency suffers as even partial success is defined as failure against an unfair standard of the president's own making—namely, inflated public expectations. Worse yet is the threat of foreign disaster as the president seeks to regain public stature by at least appearing to be bold in international affairs.

How can the problems of the personal presidency be resolved in favor of collective responsibility? Lowi rejects remedies such as the war powers or budgetary initiatives of Congress, a six-year presidential term, a national primary, and a stronger cabinet. He also disposes of several two-party "myths," but he does think a multi-party system would help. It could force the president to include Congress in his or her constituency by threatening to resolve an election in the House. Relatedly, the president would have to develop a party coalition in Congress and would thereby give new life to the role of both party and Congress.

Real parties would help to build down the presidency by affecting elections and the role of Congress and the cabinet. Collective responsibility and a more parliamentary presidency would follow. But the starting point is not structural reform. Ironically, Lowi's solution for the institutional problems of the plebiscitary president begins with an individual president's understanding of the problems and willingness to cope with them.

This is ironic because it assumes presidential behavior eventually controls institutional forms, whereas Lowi's thesis assumes the reverse. In fact, Lowi's analysis discards the individual president along with any character explanations of presidential behavior. Intuitively, one assumes the deceit inherent in the personal presidency would be made worse by some presidents than by others. Instead, the Lowi model even seems forced in places, with Richard Nixon described as "consistent, logical, and normal" (p. 179).

Lowi also seems to stress the "power invested" subthesis, although more could be said about the complications of fulfilling promises than the seesaw of public aspirations and disappointments. For instance, while he discusses the shift of power from Congress to the presidency, he says less about the atomization of Congress as this, together with what is left of its checks and balances role, affects the president's ability to "deliver."

Criticisms of a work as bold and lively as this one are inevitable. What must be said in its favor is that the analysis is insightful, intelligent, and compelling. Lowi's summation is more than an academic exercise. It is intended to make a difference. What is certain is that it will not be ignored.

JAMES A. DAVIS

Oklahoma State University

Due Process in the Administrative State. By Jerry L. Mashaw. (New Haven: Yale University Press, 1985. Pp. xiv + 271. $24.00.)

This admirable book incidentally illustrates the extent to which law professors like its author have mastered the relevant social science materials and now dominate the study of administrative law and regulatory processes. Paradoxically, the rise of behaviorism, with its presumed emphasis on grass-roots activity, resulted in the decline of the study of public administration. Administrative law was typed as formalism. Organizational case studies so beloved of sociologists were perceived as suspect in initial selection and conclusions. Decision-making analysis led many, not merely its progenitor, out of political science and into psychology. Only with the emergence of public choice analysis of regulation—a body of work well-cited by Mashaw—have political scientists come back to these fruitful areas.

In the interim, administrative law professors—always more pragmatically students of regulatory behavior (as opposed to court doctrinal analysis) than most of their colleagues—have turned to social science, especially economics, to deepen their understanding. Mashaw, who is identified with two of the prime centers of that confluence—Yale and Virginia Law Schools—gives us an analysis of the due process clause holdings of the Supreme Court. But it is also an analysis of the proper domain of courts, legislatures, and administrative agencies. It is a counterpart of roughly similar constitutional analyses by Ely, Choper, and Tribe. Essentially, *Due Process in the Administrative State* is a critique of current Supreme Court doctrine and Mashaw's suggested remedy. The critique is developed through his delineation of three models of due process and adjudication: appropriateness, competence, and dignitary rights.

By appropriateness, Mashaw includes notions derived from legal precedent and past practice, as well as legal reasoning about what is due (appropriate) process in any situation. He suggests this was the prevailing model until the Warren era. By competence, he means an approach which emphasizes interest-group balancing, response of the courts to legislated cues as to authority distribution ("positivism") and instrumental utilitarian assessment. This is traced to the welfare benefits case of *Goldberg v. Rally* (1970) and defined in *Mathews v. Eldridge* (1976). The latter case sets out yet another of the Supreme Court's myriad three-pronged tests; (1) the private interest; (2) the risk of error and the degree additional procedure would reduce error, and (3) governmental costs.

Mashaw shows that neither of these two models is complete in itself and that even the Supreme Court does not consistently decide within the parameters of its own stated tests, never mind Mashaw's constructs. The missing element is his third model—dignitary rights—admittedly a restatement of natural law, which Mashaw regards as essentially nonsense but terribly useful. The model of dignitary rights helps explain otherwise anomalous decisions and actually channelizes and minimizes judicial discretion. The model of appropriateness provides little guidance when precedents are in conflict or ambiguous. The model of competence permits, even requires the courts to smuggle in conclusions about policy that it derives by unknown methods, but which somehow are superior to legislative knowledge. In contrast, the model of dignitary rights suggests normal deference to political determination, and exceptions must be asserted in strong justifications limited to certain sectors of rights.

This is not an earthshaking argument. The strength of the book is in its compact summary of much relevant data and reasoning that goes far beyond doctrinal analysis. The writing is generally spritely and lucid, only occasionally lapsing into pretentiousness. There are good summaries of Richard Stewart's delineation of eras of administrative legitimation, the relevance of majority rule analyses for bureaucratic discretion, and Kantian and Rawlsian justifications of individual dignity as a decision rule. The introductory chapter is rich in data on the growth of due process cases and a fair smattering of public choice literature on bureaucracy is adverted to in the later chapters. In short, it is a good introduction to administrative law considerations generally, even though it has the focus of a monograph.

Its major weakness lies in the use of models, an approach law processors have overmined probably because of the pervasive influence of Packer's two models of the criminal justice system. The weakness is inherent in all ideal-type analysis which is largely an excogitated product of the model-builders' imagination. Such projected super-principles are excellent didactic devices and, no doubt, good antidotes to the lawyer's disease of super-particularistic analysis of each little case. Yet judges, by and large, do not operate with synoptic vision; they are themselves transplanted lawyers as a rule, rather than misplaced philosophers. Having created the models at will, the builders generally can manipulate them in almost any way they wish since they know their weakness and flaws as well as their intended strengths. Certainly no justice that I can recall denies that the *Eldridge* test should include "dignitary rights" though his or her emphasis or devotion varies. Mashaw's definitions embed his conclusions, as do Tribe's and Packer's.

It is intriguing to compare Mashaw's reversion to natural law thinking (in spite of his expressed aversion to it) with the revival of similar doctrine in recent English administrative law. In spite of an even stronger tradition of parliamentary supremacy and legal positivism than our own, English courts have in this past decade or so found that "natural law" doctrines of previous centuries can be used to constrain parliamentary delegation of absolute discretion. It is, however, the appropriateness model they have usually relied on, though their decisions, even more than ours, blur any such distinction. As Mashaw suggests, any argument ultimately reducible to "what kind of a government is this anyway" fits under due process and also serves to bring into play general notions of decent procedure for the English.

In so careful a book, the carelessness with cited names is jarring. Even Mashaw's collaborator in another volume does not avoid misspelling. But this, like the excessive use of the first-person singular, are minuscule flaws in a very welcome and exceptionally thoughtful work. The volume also has the potential of helping reawaken the current crop of political scientists to the fact that virtual abandonment of the regulatory area as a research focus was a grievous misunderstanding of the nature and working of modern government.

SAMUEL KRISLOV

University of Minnesota

The U.S. Intelligence Community. By Jeffrey T. Richelson. (Cambridge, Mass.: Ballinger Publishing Company, 1985. Pp. xxv + 358. $39.95, reference; $16.95, text.)

The United States intelligence community has now been written about from a number of perspectives. Memoirs and exposes still appear with regularity, but they no longer dominate the literature. One current approach focuses on personalities in charting the growth and development of the intelligence community. Figures such as Allen Dulles, Richard Helms, and James Angleton have served as the centerpiece of such studies. A second approach emphasizes the functional tasks of the intelligence community: intelligence estimating, covert action, or counterintelligence. A third approach places the study of intelligence in the broader context of the American political system. Its focus is on such questions as how and why is the intelligence community politicized, and is it under control? A fourth perspective examines the intelligence community from an organizational perspective asking questions about its structure and the impact of its structure on its performance.

Jeffrey Richelson's book falls into this last category. Where most organizational accounts of the intelligence community operate at a relatively high level of generality, this book delves into the minutiae of organizational charts, lines of responsibility, and institutional change. Moreover, Richelson adopts a broad definition of the intelligence community. His detailed account includes coverage of the military service intelligence organizations, Defense Department intelligence organizations, the intelligence components of the Unified and Specified Commands, and such civilian departments as Agriculture, Energy, and the Drug Enforcement Administration. Richelson lays out in the same painstaking detail the technological and management systems utilized for imagery and signals intelligence, ocean and space surveillance, and nuclear detonation monitoring. The book also includes excellent chapters on cooperative international intelligence agreements, and analysis and estimates.

Richelson has made a major contribution to the study of the U.S. intelligence community by skillfully bringing together in one volume a body of material that is either unavailable to most observers or would require an immense investment of time and effort to obtain. This work should not be seen as a primer on the intelligence community. The nine pages of abbreviations and acronyms following the table of contents alert the reader to the depth of detail that he or she will confront. The brevity of the discussions of intelligence in the first chapter and that of issues in the concluding chapter also indicate that this is a work most useful for those already familiar with the main themes and issues in the literature.

This book is also important for the study of the U.S. intelligence community by revealing just how far we have come over the past decade and how much more still needs to be done. Through the exhaustive use of government documents, the Freedom of Information Act, and personal contacts, Richelson is able to present a detailed if somewhat mechanical account of management practices and standard operating procedures deep within intelligence organizations. The next requirement is to add a human element to the narrative describing how the system actually works. The data needed for this is unlikely soon to be available. In discussing national intelligence organizations, Richelson must also often fall back on budgetary and personnel figures cited in Marchetti and Marks, newspapers, and magazines. Not only has the accuracy of these figures been challenged, but making meaningful comparisons is difficult. Typically, the figures are for different years, and little guidance is given as to what is included in the calculations. This lack of current information continues to plague the study of intelligence. To his credit, Richelson acknowledges these problems when they arise and seeks to steer the reader around them.

GLENN P. HASTEDT

James Madison University

Environmental Policy Under Reagan's Executive Order: The Role of Cost-Benefit Analysis. Edited by V. Kerry Smith. (Chapel Hill: University of North Carolina Press, 1984. Pp. xii + 266. $25.00.)

The great expansion of the regulatory powers and responsibilities of federal agencies in the United States during the 1970s generated increased efforts by presidents to monitor and direct the decisions and actions of these agencies. The Nixon administration focused its attention on the power of the Environmental Protection Agency to impose regulations on industry that resulted in significant compliance costs. The Ford and Carter administrations built upon the Nixon base by encouraging agencies to consider the impact of their regulatory initiatives on inflation and the economic competitiveness of the regulated industries. In 1981, the Reagan administration issued an executive order that expanded the power of the executive office of the president to review all major regulations proposed by executive branch agencies and required that agencies demonstrate that the benefits of their proposed actions exceed the costs imposed. The Office of Management and Budget (OMB) is responsible, under the Reagan order, for reviewing the economic analysis that is to accompany regulations and assuring that the proposals satisfy the cost-benefit standard, unless agency statutory law prohibits such a consideration. The OMB, however, has no direct authority to rewrite or alter the regulations themselves.

The Reagan order is part of a broader effort of budgetary, personnel, and managerial decisions and actions that seek to redirect and shape administrative activity without rewriting the laws under which agencies function. The efforts have raised much controversy and have been challenged in federal courts, in congressional hearings and proposed legislation, and in interest group and media investigations. Presidential oversight of, and intervention in, regulatory rule-making proceedings have been as controversial as the substantive regulations themselves and have raised important questions of political accountability, the politicization of scientific research and analysis, administrative and managerial efficiency, openness and public participation, and due process and fairness in the regulatory process.

A number of published studies and law review and journal articles have already been published that have focused on the politics of presidential oversight of regulatory agencies. Susan J. Tolchin and Martin Tolchin's *Dismantling America: The Rush to Deregulate* (Boston: Houghton Mifflin, 1983) and Jonathan Lash, Katherine Gillman and David Sheridan's *A Season of Spoils* (New York: Pantheon, 1984), for example, are very critical of the Reagan approach, charging that it has seriously undermined important regulatory programs. Howard Ball's *Controlling Regulatory Sprawl* (Westport, Ct.: Greenwood Press, 1984) and George C. Eads and Michael Fix's *Relief or Reform? Reagan's Regulatory Dilemma* (Washington, D.C.: Urban Institute Press, 1984) find much to criticize in the Reagan effort, but are much more sympathetic to the idea of presidential oversight of administrative decision making.

The collection of essays that V. Kerry Smith has assembled in this volume on the Reagan executive order make a significant contribution to this growing body of literature. The collection includes nine essays, from a variety of perspectives including the OMB's regulatory review office to the Environmental Protection Agency's economic analysis division to academic scholars. The major themes pursued in the essays include the evolution of presidential oversight of regulatory rule making, the theory of benefit-cost analysis, the quality of analysis that has taken place under the Reagan order, and the overall costs and benefits of imposing benefit-cost analysis in agency decision making through the executive order and OMB review. While the essays do not ignore the political issues that the executive order has spawned, they emphasize and describe with clarify the economic assumptions underlying benefit-cost analysis. Combined with other studies that give primary attention to political considerations such as the tension between congressional and presidential oversight and that between presidential review and the procedural requirements of administrative law, this volume adds much to our understanding of recent efforts of presidents to gain more control over the administrative process, and adds much to an initial and preliminary understanding of the prospects for the use of benefit-cost analysis in regulatory agencies and its effect on the quality of regulatory decision making.

GARY BRYNER

Brigham Young University

Paths of Neighborhood Change: Race and Crime in Urban America. By Richard P. Taub, D. Garth Taylor, and Jan D. Dunham. (Chicago: University of Chicago Press, 1984. Pp. xii + 264. $25.00.)

Richard P. Taub, D. Garth Taylor, and Jan D. Dunham of the University of Chicago have written an impressive but ultimately troubling book. Their objective is quite ambitious: to modify and refine one the cornerstones of American urban sociology: the invasion/succession model of neighborhood change. As their study's title indicates, they purport to emphasize the connections between crime and neighborhood racial turnover. However, in *Paths of Neighborhood Change*'s introductory chapter, which outlines the theoretical issues impinging on the authors' research agenda, the discussion of why they single out crime (as opposed to garbage or governmental unresponsiveness, for instance) as a contributor to neighborhood deterioration is at best sketchy. The remainder of the book sustains this split emphasis. For the most part the authors look at how neighborhood residents assess their communities, make judgments about whether or not to invest in home upkeep, and react to the initiatives of local government and other "institutional actors." Every several pages a discussion of crime appears, but the authors never make the case as to why this particular issue overshadows other neighborhood problems nor fully integrate the crime and neighborhood investment analyses.

The strength of this volume is its research design and methodology. The authors examine eight Chicago neighborhoods, and through a combination of direct observation, survey data, and secondary material they manage insightfully to detail each community. Following three chapters of neighborhood case studies, multiple regression and formal modeling of the survey results are employed are employed to generate explanations of local investment, neighborhood racial tipping, and crime perception as a component of neighborhood change.

By themselves, the neighborhood case studies highlight the variable paths of neighborhood change. The mathematical analyses in chapters 6 through 8 also produce some interesting insights, most prominently the ways by which housing market conditions can affect the likelihood of neighborhood racial turnover. As is typically the case, the authors make some arbitrary judgments in manipulating and interpreting their data, and these judgments have bearing on their analysis. As both a dependent and independent variable, housing investment is assumed to bode well for a neighborhood, when in reality it is a more ambiguous activity. Homeowners rigging their houses with sophisticated burglar alarms and front door gates, or surrounding their yards with high fences, may not build the confidence of their neighbors. The authors are puzzled at what they interpret as the racial prejudice of blacks directed at other blacks—specifically their fear of neighborhoods becoming totally black—but it does not occur to them that black residents of a city like Chicago associate such segregated neighborhoods with redlining and reduced city services. What the authors view as racial prejudice might alternately be called reasonable apprehension. Finally, Taub, Taylor, and Dunham anchor their discussion of individual investment in theories of collective action, yet they do not connect this approach to the functions of community organizations, which succeed—on one level at least—by constraining the free-rider temptation.

The foregoing problems are real, but it should be repeated that they occur within the context of a generally rigorous and creative analytical operation. What troubles this reader most about *Paths of Neighborhood Change* are some of the conclusions the authors draw. On one hand are perfectly sensible correctives to some observers' faith that local organizations, by themselves, can save neighborhoods, or the analogous thinking that the problems of frostbelt cities can be solved through purely local initiatives. Yet the authors also support neighborhood triage, when there is absolutely no reason to suppose that the residents of written-off neighborhoods would receive the job counseling, psychological supports, and financial aid requisite for starting life anew in some other location. Similarly, the authors hold to a faith in the benevolence of institutional actors as agents of neighborhood stability. This flies in the face of the experience of about half their Chicago neighborhoods, in which institutional actors have sought to speed or impede neighborhood change for reasons often quite insensitive to the welfare of local residents. Oddly, in a study that goes so far in describing how distinctive are the courses of neighborhood evolution, the authors themselves fall prey to reifying the concept of neighborhood and forget that people with lives, jobs, expectations, and fears are the neighborhood. Policy should be informed by the needs of these people and not by the idea of neighborhood improvement.

LARRY BENNETT

DePaul University

Democrat Versus Democrat: The National Party's Campaign to Close the Wisconsin Primary. By Gary D. Wekkin. (Columbia: University of Missouri Press, 1984. Pp. x + 253. $23.00.)

When Democrats circle the wagons, says the old joke, their rifles aim inward. In the wake of wagons circled—for an internecine shootout that isn't over yet.

Democrat Versus Democrat describes one of the battles in this now eighteen-year war—that between the Democratic National Committee and Wisconsin Democrats over the latter's beloved open primary. Wekkin has captured it with a rigorous eye and a lively pen. The book begins with an important theoretical scorecard, telling the players' positions. Of all the questions raised in this first chapter—federal, factional, legal—perhaps none is more important than the democratic: "the question of whether democratic government means direct, popular participation in decisionmaking—or popular control over those who decide" (p. 12). The first, the "classical" position, "has been the credo of most Progressives and McGovern-era reformers and of the many Wisconsin Democrats who have opposed implementation of the DNC's closed-primary rule." The alternative cudgels are carried by "revisionist democrats such as E. E. Schattschneider and some advocates of 'responsible party government'" (p. 13).

Subsequent chapters illustrate mainly that, alas, players did not often choose their stands on such elevated grounds. For example, some reform Democrats at the national level supported closure largely because George Wallace had done well in Wisconsin's "Democratic" primary by dint of Republican and independent votes. Regulars also supported closure, thinking (wrongly) that crossover voting had robbed Hubert Humphrey of a 1972 victory in the crucial Wisconsin primary. Wisconsin party-organization members, who had aspirations within the national party organization, were more easily resigned to closing the primary than were Wisconsin politicians, who had to face Wisconsin voters. Jimmy Carter saved the state's open primary in 1980, when polls showed he would do well in it. But presidential candidates spurned the primary in 1984, when it fell too late to be vital to them. And so it went, in a dismal parade of calculations about whose ox would be gored, whose ax would be ground.

It is not a criticism of this book, but a tribute to it, to say that there is plenty of room for argument here. For example, in rendering the revisionists' position as one of the conflicting theories of democracy, Wekkin cogently shows that both makers of and participants in the debacle at Chicago in 1968, Democrats shouted "reform!" and, sure enough, the primary elections have tended to be "an elite few" (p. 14). But he doesn't recall this argument in the final chapter, where he makes the revisionist position his own. There, he truncates the argument after saying that the people have trouble governing in the absence of responsible parties. Of course, *some* people do govern after *the* people default out of primaries, and it would seem logical to say again who they presently are, to add that they are demographically and attitudinally unrepresentative of the party constituency and its interests.

Wekkin does not do this, because, in the end, his animus toward presidential primaries is somewhat tepid. On one hand, he opposes the open primary because it "lessens party responsibility" (p. 182), and he "hopes for more such triumphs for the revisionist view" (p. 189). On the other hand, Wekkin is satisfied that the existing system of primaries and caucuses open to any "self-designated adherent" does assure a "greater degree of internal cohesion and responsibility" (p. 184). That is perhaps too sanguine a conclusion when, for example, over half the self-designated Democrats in the country don't know the difference between the Democratic and Republican positions on the watershed questions of government responsibility for insuring employment or an acceptable standard of living. One can argue that when the Democrats "opened up" the delegate selection process to an electorate unfamiliar with party ideology, they wrought a babel of Madison-Avenue slogans, not a responsible party. And the unhappy irony is that no group has been so disconcerted by this confusion of tongues as working and lower-class Americans—the bedrock of the Democratic party.

But again, this is not a criticism. It is just the kind of question one expects to arise from a good read—the kind provoked by the work of an active mind on an urgent issue.

CALVIN F. EXOO

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Aging and Public Policy: Social Control or Social Justice? By John B. Williamson, Judith Shindul, and Linda Evans. (Springfield, Ill.: Charles C. Thomas Publishing Co., 1985. Pp. 332. $34.75.)

A consideration of this book can usefully begin by pointing out that essentially the same group of joint authors several years earlier produced a comparable book in the same general field. That work, *The Politics of Aging: Power and Policy* (1982), developed many of the themes that are to be found in the work presently under review, but was fundamentally different in key respects. In *The Politics of Aging* the unifying theme was *political influence:* heavy stress was placed on the hopeful prospects for "senior power" and the potential for "coalition formation" between this country's organized elderly and other constituencies of disadvantaged citizens. While the 1982 book was careful to point out that there could be no absolute assurance of growing political strength for the elderly over the next 50 years, the authors still could voice optimism that the elderly's political resources "will translate into increases in autonomy and political influence," and even could venture the possibility that "the increase [in influence] will be much more substantial than even the most optimistic of today's pro-aging advocates anticipate" (p. 264). While the book did contain some discussion of the less upbeat theme of the *social control* of the elderly, this was only a subsidiary motif.

In the book presently under review the authors not only shift their focus from the elderly's influence and power potential toward the politics of old age programs and policies, they also adopt the social control motif as their dominant theme. Their frame of reference is now firmly neo-Marxian, and their stance toward aging policy in America becomes consistently radical and critical. While many factors presumably contributed to this change in emphasis, one factor clearly was the marked deterioration in the material and psychic circumstances of large numbers of elderly Americans since the period of the 1970s. Thus, the note of pessimism that pervades the present work to some degree reflects the authors' sense of the elderly's recently-diminished prospects—a point that is strongly underscored in their analysis.

Williamson, Shindul and Evans develop the thesis that the elderly have become a leading example of how "enlightened bureaucratic management" can replace the controls once wielded by the primary group and the coercive powers of the state as sources of social control. They go on to insist that such bureaucratic manipulation, combined with a no less coercive medical and psychiatric model, now largely defines the limits of old people's possibilities and potential. In arguing their case, the authors draw on the findings of other radically-inclined analysts who have earlier written on American policy in aging, especially Carrol Estes, William Graebner, John Myles and Laura Olson. However, whereas the other radical authors have tended to concentrate on one or another *facet* of public policy—for example, age-based social services in the case of Estes, or retirement policy in the case of Graebner—the present authors adopt a more systematic approach, with no major aspect of American aging policy left unexplored. Their approach is essentially encyclopedic, rather than idiographic, a fact reflected in the exceptionally long (50 page) bibliography at the end that is needed to identify properly the works cited in the text.

Among the virtues of the book is its success in combining a cross-disciplinary and multi-author approach with a consistency of tone and focus. Among the three authors listed on the title page, two (Williamson and Evans) are sociologists, and the third (Shindul) is a professor of nursing. Moreover, three additional individuals have each contributed a single chapter to this twelve-chapter book, of whom two are sociologists and one a political scientist. While such diversity of authorship and disciplines might conceivably make for an unevenness of approach, this risk has been avoided; a common thread, based on the "social control" model, links the entire work and gives it coherence.

The book is not without its weaknesses, and to a large degree these are a consequence of an apparent excess of enthusiasm in developing the authors' main message. Readers who come to this source in hopes of finding a balanced treatment of programs and policies are likely to be disappointed. The tendency to see evidence of social control in every facet of aging policy in this country does not leave much room for any acknowledgement that these same policies in certain ways may actually increase the elderly's autonomy and range of meaningful choice. (This latter possibility is noted in an abstract way in the opening chapter, but the theme is not developed in the main body of the book.) Secondly, the authors' perspective may have caused them to assume something that is very much in need of proof, namely the asserted causal connection between social control and this country's prevailing capitalist system. The question of whether capitalism is the root cause of social control of the elderly would seem to be an empirical matter and not something to be assumed a priori. If the authors had adopted a cross-national, rather than a single-country, approach, the question of whether equal (or perhaps even greater) degrees of social control may exist in socialist and other non-capitalist settings might have been meaningfully raised.

These criticisms should not be taken as detracting from the book's essential strengths. For the most part, academic commentary on aging policies in this country has been at least implicitly celebratory, and in this context the recent "radical school" of program and policy analysis is serving as a useful and needed corrective. The present work deserves a prominent place within this school of analysis. While presenting little in the way of material that is in fact original, the book succeeds well in weaving together the many disparate sources that otherwise might remain inaccessible to most students of American public policy.

HENRY J. PRATT

Wayne State University

COMPARATIVE POLITICS

The French Socialist Experiment. Edited by John S. Ambler. (Philadelphia: Institute for the Study of Human Issues, 1985. Pp. 224. $27.50.)

Socialism, the State and Public Policy. Edited by Philip G. Cerny and Martin A. Schain. (New York: Methuen, Inc., 1985. Pp. 298. $29.95, cloth; $12.95, paper.)

By the time this review appears, the French electorate will have had the opportunity to give its verdict on the French socialist experiment to which both these books are addressed. It began in May-June 1981 with the promise to change life and the society in tones reminiscent of the ideological fervor of the French Resistance. In its first year and a half, the Socialist government undertook sweeping economic and social reforms. By the end of 1982, however, there was a pause followed by a retreat as the central goal of the socialists to revive and modernize the economy while providing, at the same time, full employment and sweeping welfare measures began to elude them. Why was this so? Were the original hopes simply beyond reach? Were the measures used—nationalization and massive government spending—inappropriate? Did the *bete-noire* of the French Left—the business and financial interest—intervene once more to thwart Socialism as they had done in 1935-37 and again in 1945-47? Was it a matter of bad timing? France, solidly implanted in the Common Market and the international economic order over which the U.S. presides, decided to go alone against the current of deflationary economics in West Germany, England, and the U.S.A. Were the socialist families within the Socialist Party unable to put their act together? French Socialism may have been the victim of its inherent "political pluralism" as two authors contend. Over and beyond these questions looms the larger question—and to some of the authors almost a transcendental one. If French socialism is in serious trouble, can socialism continue to be considered a viable alternative to economic liberalism? Can we still rely on the socialist blueprint to provide equality and abundance and to liberate human initiative and endeavor at the same time?

The reader will find tentative answers or, at least tentative efforts to answer all these questions and many others raised by the editors and authors in these two books. Both are excellent efforts to study the relations between state and economy—a growing preoccupation of students of comparative politics—and to evaluate public policy in France. *The French Socialist Experiment,* edited by John S. Ambler is more directly focused on state-society relations. The editor has written three of the eight essays—the first on the history of the French Left, particularly when the power—1936, 1945-47, and after 1981; on the educational reforms undertaken after 1981, and a concluding chapter on French socialism in comparative perspective—mostly in comparison with Labor Socialism in England. There are essays on Social Security under Mitterrand by Gary Freeman; on agricultural reform by John T. S. Keeler; on Immigrants and Politics by Martin Schain, on economic policy by George Ross and Jane Jensen and on decentralization by Douglas Ashford, which unfortunately is limited to the study of the law introduced by the government and as it was being debated by the legislative assemblies. *Socialism, the State and Public Policy in France,* edited by Cerny and Shain, covers a great deal more but what it gains in coverage is offset by the variety of topics chosen. It is more of a symposium, in which the various authors ran away from whatever plan the editors had in mind. There are, however, some excellent essays: Peter Hall's "Socialism in One Country: Mitterrand and the Struggle to Define a New Economic Policy;" Pierre Birnbaum's "The Socialist Elite 'Les Gros' and the State;" Kesselman's "The French Communist Party;" as well as part three, "Structural Constraints and Policy Arenas" with articles by Philip Cerny, Sally Sokoloff, Martin Schain, and John T.S. Keeler (the last two contributed also to Ambler's volume).

Virtually all essays in both these books—21 in all—live up to high scholarly standards; the books represent the best and most comprehensive analyses and evaluations of the French Socialist experiment that have appeared to date. Both must be read, and in a sense the haste of the editors to see their books published before the French legislative election of March-April 1986 is both understandable and welcome. The two together give us the most comprehensive account of various facets of the French economy and society, and they both are required reading for understanding the forthcoming elections.

Is there a general conclusion? Even though French socialism is in both books a little bit like the proverbial elephant—each author describes and analyzes what he or she has touched—the overall conclusion is inescapable. It is moribund according to some for lack of humility of the Socialist leaders who promised to change society and life; according to others because of intractable international economic forces; for some because of the internal squabbles of the French "left," though the term left remains ambiguous—sometimes the authors include the Communists and sometimes they do not. But when all is said, the salient point remains that the French left in general and the Socialist party in particular lacked a strong political base. Though George Ross and Jane Jensen are right in stating that rarely, if ever (in my opinion, the term "never" would have been more appropriate), has there been a Left government endowed with more institutional power than the Mitterrand one. Yet its political base remained shaky. A mere shift of 2-4% accounted for the Presidential and legislative victories of the Socialists in May-June 1981, with full Communist electoral support. After 1982, even this slim electoral advantage began to evaporate, and since the summer of 1984 the Communists have been sulking again in their self-imposed ghetto.

Whatever the economic and other constraints that undermined the socialist experiment, the political base was too thin from the start. And this may well be the major flaw of both books—they neglect the political base of French socialism and of the French socialist, legislative and presidential "majorities." In their effort to adumbrate the societal and economic forces at work, to dissect the various group pressures and counter-pressures, they ignore the force that binds them together or causes them to unravel—politics. There is not a single article on public opinion and opinion trends under the Mitterrand government and on the attitude of the opposition parties, including the recrudescence of the extreme right.

Roy C. Macridis

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The Emergence of a New Lebanon: Fantasy or Reality. Edited by Edward E. Azar. (New York: Praeger Publishers, 1984. Pp. x + 292. $32.95.)

Much ink has been spilled on the Lebanese tragedy and the deadly civil war which has turned Lebanon into a symbol of destruction and disintegration. What distinguishes this book, edited by Edward Azar, a specialist on Lebanese affairs and Director of the Center for International Development at the University of Maryland, from a number of others is the depth of knowledge displayed by its contributors and its focus on the socioeconomic background and historical context of the issues involved.

The authors address several critical questions. Who are the main actors on the Lebanese scene and what are their strengths and weaknesses? Where do foreign powers, mainly the United States, Israel, Syria, and the PLO, stand on the conflict? What difficulties face internal reforms? And, ultimately, what are the chances for the emergence of a new Lebanon?

Since it is difficult to do justice to all contributors in a short review, it might be better to highlight some of their significant ideas and conclusions. Paul Jureidini and R. D. McLaurin note the shift in the political and social balance of forces in the country in the years after the 1975 civil war. Lebanon, they proclaim, has "experienced a social and political revolution that is little understood either domestically or outside" (p. 6). This interpretation is worth underlining, since this "Lebanese revolution" is still in the making and is constantly surprising observers by its ever-changing phases.

Augustus Richard Norton studies a major new factor on the Lebanese scene, the Amal movement, the most conspicuous manifestation of the rise of the shi'ah as a power, noting that "whatever the future will hold for this ill fated country,' it is indisputable that the Lebanese Republic's plurality, the shi'a, will play a decisive role in shaping it for better or worse" (p. 162). He outlines, among other things, the demographic increase of the shi'a to the status of a plurality; the migration of the shi'a from the south to the north; and their poverty, especially in southern Lebanon.

If the shi'i Amal movement is the main Muslim organization in Lebanon, the primarily Maronite Lebanese Forces is the chief Christian paramilitary organization. A thorough study of this organization is undertaken by Lewis Snider who gives us an account of its rise and growth to the point where it exercises quasi-governmental power in the Christian territory under its control. Its objective is "to liberate Lebanon from Syrian, Palestinian and Israeli occupation" (p. 117).

R. D. McLaurin gives us an original study (perhaps the first of its kind in English) of the Lebanese Army, detailing its historical development; the traditional sectarian basis of its recruitment; its composition—mainly Christian officers and shi'i ranks; and its ups and downs with the Palestinians. The army, according to the author, though it is small and weak, remains solidly respected by the Lebanese population—a fact of significance for the future of Lebanon.

The critical issue of the effect of the war on the Lebanese economy is tackled by Joyce Starr who gives us some interesting but pessimistic reflections on the subject. After discussing the destruction caused by the war on the structure of the Lebanese economy, she comes to the sad conclusion that "the continuing isolation of different parts of the country—in effect, de facto partition—further compounds the stagnation which threatens to become a permanent, rather than temporary, feature of the economy" (p. 77).

If subsequent events on the Lebanese and regional scene have contradicted some of the conclusions in the book, many of the main judgments and observations remain valid. This book deserves to be read by general readers and policy makers alike who are interested in the fate and future of this critical country.

LOUAY BAHRY

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Interest Groups and Political Development in Turkey. By Robert Bianchi. (Princeton, N.J.: Princeton University Press, 1984. Pp. xiii + 426. $45.00, cloth; $17.50, paper.)

Turkey: The Politics of Authority, Democracy, and Development. By Frank Tachau. (New York: Praeger Publishers, 1984. Pp. xiii + 228. $27.95.)

Bianchi's book describes the emergence of competing means for structuring interest representation as well as tensions in Turkish politics produced by historical shifts in the relative importance of different types of groups. He examines political culture and public policy to account for variance in their influence and strategies. This includes comparative analysis of interest group activity and leadership in different economic sectors.

Essential to Bianchi's analysis is his use of Philippe Schmitter's concepts of corporatist and pluralist group structures later refined to include state corporatism and societal corporatism, with the latter characterized by groups with some autonomy while the former are dependent upon and penetrated by the state. Much of Bianchi's study is devoted to the emergence of a highly unusual and unstable mix of both state and societal corporatist groups produced by state initiatives and political party strategies amidst uneven but rapid economic development. He investigates efforts to reconcile control of tensions and demands through corporatist political strategies with maintenance of a democratic political system.

Bianchi notes that public policy in regard to interest groups has been eclectic and has in various ways—despite a political culture supportive of corporatism—contributed to representational dualism with opportunities for disgruntled group leaders to turn to alternative voluntary associations. With the transition to multi-party competition in the 1950s, large numbers of feeble, competing, and easily manipulated groups appeared. Governments encouraged this in a policy of "debilitating pluralism." From 1960 to 1980 a series of weak governments pursued societal corporatism by extending privileges to certain groups and by fostering dependence on the government while simultaneously imposing restrictions on uncooperative groups. Of particular interest in this later period are the respective pluralist and corporatist strategies of the People's Party and the Justice Party. This dualistic interest representation intensified demands for participation and distribution by shifting the demands of those who opposed results in the corporatist arena into an expanded pluralist arena which became the preserve of the political opposition. This important observation provides a significant structural explanation of the intensity and violence of Turkish politics before 1980 and offers a basis for comparison with other political systems—a comparison which Bianchi pursues at the end of his work. The Chilean case, for example, involving competition between antagonistic pluralist and corporate associational channels allied with different political forces bears considerable resemblance to the Turkish experiences with representational dualism and societal corporatism. This is comparative analysis at its best and is an important contribution to both theoretical and practical study of comparative politics. Similarly, Bianchi's comparative analysis of Turkish interest group behavior imposes analytical order on an otherwise confusing array of groups.

Bianchi attributes a degree of planning based on perceived objective interests to ruling elites which ignores the "irrational" dimensions of political behavior. More importantly, despite a disclaimer concerning the "ecological fallacy" and problems in using aggregate data, it appears he finds other kinds of data unacceptable. Not only does he dismiss local community organizations as "politically insignificant" (p. 155), he attributes the "retarded state" of the study of the social bases of Turkish political parties in large measure to the restricted opportunities to conduct public opinion surveys (p. 154, n. 14). Similarly, he pays little attention to the literature on groups in local politics. This literature would have helped balance Bianchi's skewed "macro" view and provided a clearer picture of associational dynamics. For Bianchi the bottom line is aggregate data and survey research even at the community level (pp. 150-51). Political science includes a diversity of perspectives, and Bianchi's view seems dogmatic.

The author's relatively uncritical use of the newspaper *Cumhuriyet* as a source of information is disturbing. For more than a decade, *Cumhuriyet* has been a spokesman for the radical left and has held a conspiratorial view of Turkish politics. Another, more positive, observation is that this work is of the same genre as *The Turkish Political Elite* by Frederick Frey. It relies heavily on statistical analysis, and is likely to be as important to interest group analysis in Turkey and elsewhere as Frey's work is to elite analysis.

Like Bianchi, Tachau is concerned with political change in Turkey. He studies this and modernization in terms of the concepts of authority, democracy, and development. Although the work is not intended to be comprehensive, it provides a wealth of information, a skillful synthesis of much of the best literature on Turkish politics, and an insightful analysis of important aspects of the Turkish political experience including constitutional changes, political participation, economic issues, and foreign policy. Tachau's intimate knowledge of Turkey is apparent throughout, and although he remains alert to general trends and issues, he studiously describes and provides examples of important details of Turkish political life. He is at his best in bringing together and analyzing the case study literature on rural Turkey—literature to which Bianchi gives little attention. His description of the critical position of provincial towns and the rich and varied dynamics of their group politics provides the reader of both of these books with a useful reminder of the perils of aggregate analysis and the temptation to reify categories amenable to statistical analysis. His instructive study of economic issues addresses income distribution, standard of living, and social welfare and their relationship to Turkish political culture and particular political events.

In Tachau's treatment of foreign policy, little attention is paid to the importance of Turkish "guest workers" in Western Europe. Turkey's improving relations with the Muslim world are discussed but the interesting related issue of Turkish-Israeli relations is largely ignored. His emphasis on the external determinants of Turkish foreign policy leads him to overlook the foreign-policy implications of the ideological struggles caused by the social and economic transformation he describes so well. Anti-American sentiment, for example, may have been exacerbated by particular American policies, but it was hardly created by these policies. This said, Tachau's excellent description of the Turkish role in the Cuban missile crisis must be noted. His discussion of Turkish perceptions is a perspective rarely found in studies of that crisis. Students of comparative politics will find this work a thoughtful overview of Turkish political development and specialists on Turkey have been given a stimulating synthesis and interpretation of much of the literature on Turkish politics.

ARNOLD LEDER

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The Consensual Democracies? The Government and Politics of the Scandinavian States. By Neil Elder, Alastair H. Thomas, and David Arter. (Oxford: Martin Robertson, 1982. Pp. xii + 244. $49.95, cloth; $11.95, paper.)

The authors' expressed purpose in *The Consensual Democracies?* is to provide students with a contemporary study of Nordic politics in English. It replaces the woefully dated five-country comparisons of Nils Andren and John H. Wuorinen written in the 1960s (Nils Andren, *Government and Politics in the Nordic Countries: Denmark, Finland, Iceland, Norway, and Sweden* [Stockholm: Almqvist & Wiksell, 1964]; and John H. Wuorinen, *Scandinavia* [Englewood Cliffs, N.J.: Prentice-Hall, 1965]). It makes more useful for instruction collections of essays such as Erik Allardt, editor, *Nordic Democracy* (Copenhagen: Det Danske Selskab, 1981) which is a smorgasbord of topics of varying difficulty and which lacks an organized perspective for studying the region.

A brief political history of the Nordic countries is presented in the introduction. Chapter 1 raises the question of the origins and viability of Scandinavian consensual politics. A three-dimensional definition of consensus is offered: (1) the degree of opposition to the framework of rules and regulations for conflict resolution; (2) the level of conflict or the intensity of societal cleavages and the exercise of political power; and (3) "the degree of concertation in the gestation of public policy" (p. 11). Chapters 2 and 3 outline the structuring of mass politics in the region and the patterns of party conflict from 1870 to the late 1970s in the tradition of Stein Rokkan. A "nuts-and-bolts" description of political institutions is found in chapter 4. Chapter 5 addresses parts 2 and 3 of the consensus definition for Denmark, Finland, and Iceland as separate cases and comparatively for Norway and Sweden. Chapter 6 adds an international dimension by outlining consensus and cooperation in international security matters and regional cooperation through the Nordic Council. And the epilogue ponders political trends and prospects.

The authors do a commendable job in juxtaposing the structure and development of five, "five-party" systems in chapters 2 and 3. There is a nice mix of historical narrative, tables, and a sensitivity to dominant paradigms of Scandinavian political development. There is detailed description of the political conflicts of the 1970s and their resolution (chap. 4). Noteworthy is the thorough discussion of Iceland which distinguishes this book from most Scandinavian comparative studies. Students interested in pursuing further work on the area will find the primarily English bibliography helpful, particularly with respect to Sweden. More Norwegian citations would have been welcome.

The description of the institutional framework and international politics is less well balanced because more information could have been added. The conflict management role of the courts and a limited power of judicial review, especially in Denmark, Iceland, and Norway, is omitted. The reader obtains substantial information on the structure of institutions, electoral rules, timing of institutional innovation, etc., but little on the political culture within the institutions. Missing is the nuance found in Johan P. Olsen's articles compiled in *Organized Democracy* (Oslo: Universitetsforlaget, 1983) on Norwegian elite behavior and their management of political conflict. No mention is made of the restructuring of local government and the experiments to promote consensus by decentralizing decision making and strengthening grass-roots democracy. The resurgence of conservative political forces is underestimated.

Trying to compare five countries is a difficult task, especially in a limited space and on a multitude of subtle dimensions. Thus, one should not be overly critical of the omissions. The book is primarily a description of a set of political events and structures in Scandinavia coherently presented around the question of political consensus. The book is well written. And it fills a significant gap in instructional materials on Western Europe and orients scholars to a rich body of data and studies on the Nordic democracies.

ROBERT B. KVAVIK

University of Minnesota

Islam and Politics. By John L. Esposito. (Syracuse, N.Y.: Syracuse University Press, 1984. Pp. xviii + 273. $12.95.)

This study of the role of Islam in politics should prove useful to the layperson or the professor seeking one short book that recounts and explains the major religio-political events that rocked the Islamic world in the late 1970s and early 1980s—for example, the rise and evolution of the Islamic Republic of Iran, the seizure of the Grand Mosque in Mecca, and the assassination of President Sadat.

Esposito limits the geographic scope of his study to the Middle East (from Morocco to Pakistan, excluding Afghanistan), and presents it in the form of a chronological narrative. Chapter 1-3 provide the historical background or context of contemporary Islamic politics. They deal successively with the relationship of Islam to politics in early Islamic history, the nature and legacy of the various movements of Islamic revival and reform in the eighteenth and nineteenth centuries, and the role of Islam in the anti-colonial independence movements and nationalist ideologies of the twentieth century. Chapters 4 and 5, comprising half the text, are more current. Chapter 4 discusses the diverse state- and nation-building paths followed by newly independent Muslim countries in the quarter century after World War II. Using case studies of Turkey, Saudi Arabia, Pakistan, Egypt, and Iran, it analyzes the nature and influence of two major Islamic movements, the Muslim Brotherhood and the Jamaat-i-Islami. Chapter 5 explores the sources and expressions of the Islamic resurgence in the 1970s and 1980s through an examination of the use of Islam by both incumbent governments and opposition movements in Libya, Pakistan, Iran, and Egypt. The study concludes with a short chapter that analyzes the problems and prospects of contemporary attempts to establish modern Islamic states and societies.

Esposito is a knowledgeable and sympathetic student of Islamic politics. He is sensitive to the staggering socioeconomic problems that have helped to trigger the Islamic resurgence—problems shared by most less-developed, modernizing countries—and he takes seriously and is respectful of the efforts of Muslim thinkers and activists to find culturally appropriate solutions to these problems. These matters are presented from the perspective of the developing countries involved, their histories, and the history of Islam—not from the perspective of international politics and the concerns of American foreign policy. The presentation is straightforward and quite free of jargon.

This book is a survey, however, and not a scholarly monograph. It contains little by way of conceptual rigor or theoretical insight to interest the specialist. Well-known facts and unexceptional explanations are frequently repeated, sometimes word for word and even on successive pages, rather than clarified or elaborated. Moreover, the text contains confusing conceptual and factual slips. The repeatedly made distinction between "Islamic" and "Muslim" states is never really explained. Is there a useful distinction to be made? If so, what is it? On one page the reader is told that in 1949 there were 500,000 members in the Muslim Brotherhood and on the next page it is put at 2,000,000. Which is it?

Finally, this book cries out for editing. Confusions like those cited above should have been caught and sorted out. Careful editing would have eliminated the unnecessary repetitiveness, a luxury that short books on big topics cannot afford. Most annoying, many sentences make no sense apparently because words were deleted or substituted and punctuation marks were omitted or misplaced as this work went from the typed to the printed page. The study is disappointing in part because of Esposito's qualifications. Chairperson of the Department of Religious Studies, College of the Holy Cross, he has in recent years authored or edited at least four other volumes on various aspects of Islam and the modern world.

ROBERT BURROWES

New York University

Political Elite Recruitment in the Soviet Union. By Bohdan Harasymiw. (New York: St. Martin's Press, 1984. Pp. xxii + 277. $27.50.)

Elite Studies and Communist Politics: Essays in Memory of Carl Beck. Edited by Ronald H. Linden and Bert A. Rockman. (Pittsburgh: University of Pittsburgh Center for International Studies, 1984. Pp. xii + 352. $34.95.)

As Sovietologists ponder the strategic moves of Mikhail Gorbachev, it becomes increasingly apparent that his success will rest upon his ability to confront Brezhnev's legacy of elite immobilism. To grasp fully the significance of this challenge and to translate it meaningfully for the western public and policy makers alike, Sovietologists would be well advised to examine afresh their assumptions and models of political elite recruitment in Communist systems. An excellent place to start is Linden and Rockman's *Festschrift* to Carl Beck, and Bohdan Harasymiw's thorough empirical analysis of political elite recruitment in the USSR.

The readings in Linden and Rockman's volume will go a long way toward advancing research in this area down the path of rich theory building and useful empirical analysis—two objectives which Carl Beck would have surely found an appropriate dedication to his scholarly efforts. The eleven papers in this volume are remarkably consistent in their conceptual breadth and clarity of presentation. While each chapter is enlightening, arguably the most seminal chapters in the volume are those of Valerie Bunce ("Of Power Policy and Paradigms: The Logic of Elite Studies"), and William A. Welch ("Political Elites and Public Policy"). Both chapters capture the general points developed throughout the volume: The need to view the environmental context within which political elites function, and the need to conceptualize the interaction between the environment and the structural constraints confronting the political elite. Bunce turns our attention specifically to the dynamic of the political economy, while Welsh explicates the utility of combining different levels of analysis in our study of the linkage between communist leadership preferences and policy outputs.

Despite the volume's overall richness, one grows increasingly uneasy moving through these papers. It becomes apparent that on the surface the frameworks for analysis promise much for future research, but not far beneath the surface it is equally apparent that the actual prospects remain limited. Indeed, it is interesting to note that the only empirical treatment in the volume is directed toward Yugoslavia.

If one comes away from Linden and Rockman with some doubts as to just how far we can go toward putting our designs to work for us, then turning immediately to Harasymiw's study provides the reassurance that our conceptualization can lead to fruitful empirical verification. Harasymiw's principal task is to address the question: How can the Soviet Union remain so apparently stable while confronted by the turbulence in Poland and the inevitable pressures which bureaucratic modernization and industrial development create at home? To answer this question, Harasymiw examines the means of adaptation practiced by the Communist Party of the Soviet Union (CPSU). Specifically, he leads the reader through careful discussions of the cadre selection process, the changing role of the CPSU in a modern bureaucratic state, and the party's adaptation relative to the rate of societal change coloring the Soviet Union.

The volume provides a rich collection of social scientific research with variables carefully operationalized and hypotheses and alternative hypotheses logically presented and considered. However, the most basic shortcoming of the volume is his concluding section. After a thorough exploration of the political recruitment process from Khrushchev to Brezhnev, the reader is left hanging when the author offers very little speculation about the challenges confronting the post-Brezhnev political elite. His data confirm that while the Brezhnev era was a period of party "proletarization," the process was not only imbalanced, favoring certain worker occupation groups (i.e., skilled workers and white-collar employees) but reflective of a gender preference as well. Furthermore, during Brezhnev's long tenure, the recruitment into the party activist positions took on an affirmative action character. This has meant that the technical intelligentsia have been largely bypassed relative to workers and peasants. Further restricting the technical intelligentsia's recruitment into active leadership positions has been the slowing of the cooptation process which allows non-activists to bypass normal career paths. Combined with a decline in the growth of party recruitment generally, the Brezhnev years have left Gorbachev with a legacy of "proletarians" occupying disproportionately political activist roles in the CPSU, while the intelligentsia remain largely closed out and presumably anxious for their chance at leadership. Here the reader is left wondering what it all means. This is unfortunate, for Harasymiw's data and presentation go much further than most studies toward explaining what exactly Brezhnev's legacy of the "stability of cadres" process is and how it works, and why it was built. It would have substantially strengthened the volume if the author had offered a few more thoughts on what the institutionalized elite recruitment process means for Gorbachev and his generation.

Taken together, these two volumes provide the discipline with some of the most thoughtful and illuminating material on communist political elite recruitment presently available in the literature. Both should be of much value to Sovietologists and comparativists engaged in sorting out the policies of Gorbachev and the post-Brezhnev Soviet political elite. Perhaps more importantly, both volumes are a tribute to the maturation and sophistication of Soviet studies, helping to dispel the subdiscipline's reputation of nescience.

JOHN D. ROBERTSON

Texas A&M University

Iraq: Eastern Flank of the Arab World. By Christine Moss Helms. (Washington, D.C.: Brookings Institution, 1984. Pp. 215. $28.95, cloth; $9.95, paper.)

Since September 1980, Iraq has been engaged in a dangerous and costly war with neighboring Iran, the outcome of which may affect the energy economy of the West and the prospects for political stability in the Persian Gulf. It is through the Persian Gulf that about two-thirds of the world's exportable oil flows via the principal sealanes. Iraq holds oil reserves second only to those of Saudi Arabia in the Middle East. The concentration of oil resources in the Persian Gulf, and the turmoil that affects the Middle East have created a major policy problem for energy-consuming industrialized nations.

In light of the importance of the Persian Gulf region, and of Iraq in particular, Helms examines the fundamental determinants of Iraqi policy—influences steeped in history and the geographic configuration of the country. She focuses on the forces that influence Iraqi policy formulation and evokes the perspectives from which the Iraqi government views its problems and priorities.

The book is in three parts and seven chapters. The parts are on domestic politics, ideological considerations, and conflict with neighbor. The seven chapters describe and assess the key elements in Iraqi policy. They deal with land and people; the emergent state; the years of growth; party organization; the Iran-Iraq war; a prognosis on the war; and the future of Iraq.

In her introduction Ms. Helms regrets that "it is ironic that Iraq, a key player in the drama unfolding in the Gulf, receded from the attention of senior officials in Washington after the overthrow of the last openly pro-Western government in Baghdad in 1958" (p. 1). She is right that because of lack of understanding and knowledge of the Middle East region the American government invites more trouble for its policy in the area.

Part three is the central aspect of the study which analyzes the Iraq-Iran war and offers a prognosis. It is neither a chronological account nor a military assessment of the war, but rather a description of the sources of conflict and of the decisions the Iraqi government made in light of its presumed objectives. Helms undertook a formidable task to examine the causes and foreseeable consequences of the war. Although she was able to visit Iraq twice to interview some of the officials, her "knowledge of Iraqi decisionmaking in the war has been sharply limited by the secretiveness of the Iraqi military" (p. 135). Yet she compiled much of the latest information on the war and corroborated the fact that unless the war ends in disaster for Iraq, that country will remain a vital force in the Middle East. Her treatment of the Iraqi position on the war is adequate.

The concluding chapter points out the size of Iraq's oil reserves and the potential for diversifying its oil-based economy. The war has sparked renewed concern about the possibility of closing the Strait of Hormuz by Iraq's enemy—Iran. Helms outlines Iraq's policy regarding the war quite well, but she fails to provide enough information in her observations about the possibility of closing the strait and its consequences on the world economy.

This slight criticism of the otherwise well-researched and documented study made possible by grants from the Rockefeller Foundation and the U.S. Department of Energy should not detract from the contribution Helms intended to make in the ever-growing literature on Middle Eastern affairs.

Helms, a research associate at the Brookings Institution, uses a simplified form of transliteration of Arabic words, chosen for the general readers rather than specialists. This makes the study of the volume much easier for the Western readers. All in all, this book is an extremely useful one for those interested in a study of Iraq.

SHEIKH R. ALI

North Carolina Central University

Theatre and Cultural Struggle in South Africa. By Robert Mshengu Kavanagh. (London: Zed Books Ltd., 1985. Pp. xv + 330. $26.25, cloth; $10.25, paper.)

Constitutional Reform and Apartheid: Legitimacy, Consociationalism and Control in South Africa. By Laurence J. Boulle. (New York: St. Martin's Press, 1984. Pp. xii + 270. $27.50.)

Book length studies of South African playwrights and their works are rare, so one welcomes any good study of the South African theatre scene, regardless of its obvious biases. Kavanagh was born and raised in South Africa, but it was at Oxford that his politics moved Left, as far as Mao in the Third World, he says. And he became convinced "that theater should be used in South Africa as a means of mass organization, mobilization and conscientization" (p. x). He returned home to teach at the University of Witwatersrand, and became very active in the local avant-garde (multi-racial) theater scene in the 1970s. It is with this background that he shares his insights on off-off-Broadway theater there and the people engaged in it.

Kavanagh states in the introduction that he follows the ideas of Marx and Engels that art must "be evaluated from the position of the revolutionary class," and of Lenin: ". . . for the socialist proletariat, literature can not be a means of enriching individuals or groups; it can not, in fact, be an individual undertaking, independent of the common cause of the proletariat. . . . Literature must become *part* of the common cause of the proletariat" (p. xiii). From this perspective he undertakes an analysis of theater in the Johannesburg area up to 1976. (Many interested in South African playwrights, especially the internationally successful Athol Fugard, will wish the study had been made more current, as too many excellent works are thus eliminated.)

Kavanagh focuses his analysis on four plays: *No Good Friday* by Fugard; *King Kong*, which was produced by the Union of South African Artists; *Too Late* by Gibson Kente, and *Shanti* by Mthuli Shezi. In each instance he relates the history and class origins before synopsizing it, and analyzes it from the angles of "culture and ideology," politics, aesthetics, and language. It is in this analysis that the author is at his best, quoting liberally from the plays to provide excellent contemporary literary analysis.

On the other hand, Kavanagh clearly feels that a significant progression is made in expressing class consciousness from the beginnings in Fugard's multi-racial production (based on liberal, middle class values) to Shezi's *Shanti*, which is an attempt at a specifically political use of the theater by a playwright so devoted to the Black Consciousness movement that he becomes vice-president of the Black People's Convention. The two plays in the middle show an evolution toward the attempt of blacks to establish hegemony over their own culture rather than accept being coopted into the liberal, middle-class white theater. Despite occasions when the author lets his class-based analysis get in the way of reasonable interpretation, the book contains a great deal of compelling analysis of both South African theater and the society from which it springs.

Boulle's book is described by its author as "a monograph on South Africa's constitution in the light of consociational theory (p. vi). Indeed, it will appeal to some students of the southern African situation because the second half of the book is a constitutional/legal analysis of the 1977 and 1983 constitutions. The latter is contained in its entirety in the appendix. Boulle is an associate Professor of Law in the University of Natal. His study is clearly legalistic and has limited political realism. For example in his examination of the positions of political groups active in South Africa on the constitution issue, he fails to include the ANC or the UDF.

Boulle attempts to weigh the merits of consociationalism as a solution to the constitutional problems of South Africa. In doing so he draws first on a long list of political theorists right out of every Intro. to Political Science syllabus. His analysis of liberal democratic majoritarianism concludes that its basis is electoral competition in which a minority can always hope to gain a majority vote. But in a plural society where a group such as South Africa's white population is out-numbered by six to one, majority rule, he claims, would doom the whites to a position in a permanent minority. Thus, adversarial democracy as we know it can never work in South Africa. (Of course this analysis assumes that political division will be invariably along racial lines, an oversimplification to say the least, and a position that does not correspond to the political history of South Africa or with current realities.)

Thus, advocates of consociationalism challenge the identification of democracy solely with majoritarianism, preferring to substitute their own position. Using the consociational model of Dutch theorist, Arend Lijphart, Boulle explains that this approach has four elements: (1) a grand coalition of prudent, pragmatic elites representing all major groups, (2) mutual veto, (3) proportionality in governing institutions, and (4) segmental autonomy. While even Boulle recognizes the historical failures of some of these principles, he finds it to be the only feasible alternative that could be acceptable to all sides. He then backs away from that position, however, and ultimately seems skeptical that it could work in South Africa. As a beginning, he is not even sure that one could put together a core of leaders for a grand coalition who could carry their constituents with them. In any case, he notes that the 1983 constitution appears to have moved away from consociationalism rather than toward it.

JOHN J. GROTPETER

St. Louis College of Pharmacy

Choosing an Electoral System: Issues and Alternatives. Edited by Arend Lijphart and Bernard Grofman. (New York: Praeger Publishers, 1984. Pp. xii + 273. $32.95.)

In 23 short chapters, separately written by 22 authors (including the editors), Lijphart and Grofman seek to respond to a renewed interest in electoral reform. They regard their volume as unique in that it brings together "all of the basic issues and all of the key alternatives for electoral choice, presented by scholars with widely varying electoral system preferences" (p. 3). In this respect, the book is distinguishable from *Democracy and Elections: Electoral Systems and Their Political Consequences*, edited by Vernon Bogdanor and David Butler (Cambridge: Cambridge University Press, 1983), which avoided advocacy and presented excellent specialized analyses of particular national experiences around a principal theme stressing the determining role of political circumstances. True, some overlap occurs. A few chapters in the Lijphart-Grofman volume also usefully review national experiences, and two authors, David Butler and Richard Rose, contribute here as well as to the earlier work.

Choosing an Electoral System is striking because of its variety of viewpoints. It includes chapters by long-time critics of proportional representation (PR), notably Ferdinand A. Hermens; by established champions of PR, like Enid Lakeman and George H. Hallett, Jr.; by proponents of systems differing from both PR and single-member, simple-plurality election, exemplified by Steven J. Brams and Peter C. Fishburn; and by scholars who treat the electoral system as relatively unimportant, as do Richard Rose and William Riker. Almost every author begins with a reconceptualization of electoral systems and their roles in democratic government. Repetitious as this becomes after a few chapters, it is no doubt necessary for the development of each argument. At any rate, the chapters are so brief that a reader's interest is unlikely to disappear even if one shares the view that electoral systems are not major causal factors in the success or failure of democratic regimes. Such a reader will, incidentally, be happily confirmed in that view by Rose and Riker. In his customarily trenchant style, Riker puts the point starkly: "Constitution writers might as well adopt the electoral method that is most comfortable for the people who will use the constitution they produce, plurality or PR as the case may be" (p. 110).

From the record, the "most comfortable" method for most nations appears to be the one adopted about a half century ago. As Dieter Nohlen's analysis (pp. 217-24) shows, basic changes have not often taken place in the last fifty years. France was an exception even when the book was written, for the Fifth Republic's single-member, double-ballot Assembly election represented a return from the Fourth Republic's PR to the method customary in the Third Republic. Now that the Socialist majority seeks to cut its future electoral losses by moving again toward PR, the French exception is still more conspicuous. But for the very reason that France has so often changed methods, its return to PR would strike most of us as less portentous than the adoption of PR by Britain. Yet it is that bastion of single-member, simple-plurality election where change has lately been most widely and seriously discussed. Furthermore, so detached a scholar as David Butler believes it unlikely that "first-past-the-post voting for the Westminster Parliament will last to the end of the century" (p. 229).

Among the numerous advocacies in this volume, one of the most curious is the suggestion by both Maurice Duverger (pp. 36-37) and R.J. Johnston (p. 69) that the U.S. House of Representatives be elected by PR. Presumably, third and fourth parties would then be much more likely to elect members, but it is not clear that the result would be greater diversity in representation of opinion or interests than is now achieved through our loose and porous Republican and Democratic parties. It may be granted that multipartism in an American legislative body does not, as in a parliamentary system, make for a multiparty executive coalition; hence, one cannot argue that the decreased chance of a legislative majority party, under PR, has possibly baneful effects on executive stability. Nonetheless, the absence of a majority party in the House of Representatives could make it significantly more difficult to conduct legislative business in a constitutional system where the Congress is expected to exercise great authority of its own. The looseness of each of our major congressional parties, while accommodating diverse policy preferences, does not preclude effectiveness in organizing each house and determining its procedures.

LEON D. EPSTEIN

University of Wisconsin

Studies of the Structure of National Elite Groups. Edited by Gwen Moore. Research in Politics and Society: A Research Annual. Volume 1. (Greenwich, Conn. and London: JAI Press, 1985. Pp. x + 283. $23.75.)

The eight papers in this volume are for the most part based on elite studies done in the early 1970s and already reported on by their authors in various other publications. All but one focus on a particular country and offer a descriptive analysis of what is referred to as the "structural integration" of its national elites—broadly conceived as social homogeneity and/or political agreement among the incumbents of leading positions in state and society. While the theoretical perspectives and methodological approaches employed by the different authors vary a good deal, social homogeneity is generally deduced from demographic data and elite consensus from opinion surveys.

The country chapters on elites in the United States (Barton), West Germany (Hoffmann-Lange and others), Australia (Highley and Deacon), Franco Spain (Baena del Alcazar and Pizarro), Israel (Lisak), and Colombia (Ocampo-Zamorano) will mostly interest area specialists who are not familiar with the original studies. Unfortunately they bear little or no relationship to each other, though the editor and most of the contributors collaborated for many years in the International Opinion Makers Project. Here one wishes for a more precise common analytical framework, for less conceptual confusion, and, above all, for some sort of comprehensive introductory or concluding chapter by the editor.

In the only systematic cross-national contribution to the volume, Field and Highley claim to put forward a novel elite theory with far-reaching policy implications for the United States and other Western countries. In modern "bureaucratic" societies political stability—defined negatively as the absence of violent seizures of governmental power—is said to rest on structural integration and a consensus on proper political procedures among the occupants of top "strategic positions" in "powerful organizations." Their admittedly superficial survey of national elites and political stability in some eighty countries in recent times persuades the two authors that what they consider the persistence of stable political institutions under all sorts of regimes is first and foremost a function of enduring and stable elite integration. Since they find the latter condition to be the normal state of the elites in practically all countries, Field and Highley conclude that there is little that Western countries can do to promote stable representative institutions around the world. Such policies are termed "unrealistic" and more likely to do harm than good because they allegedly concentrate on nonelite conditions to bring about desired changes that, in the last analysis, are said to depend on the disintegration of both elite structures and elite consensus.

This reviewer finds these arguments neither particularly original nor especially persuasive. Others, such as Paul Keczkemeti in *The Unexpected Revolution* (Stanford University Press, 1961, p. 149), have similarly argued that regime stability depends on the unity of the ruling elites. And the evidence marshalled by Field and Highley here seems far too tenuous to support their rather emphatic policy conclusions.

LEWIS J. EDINGER

Columbia University

Latin American Democracies: Colombia, Costa Rica, Venezuela. By John A. Peeler. (Chapel Hill: University of North Carolina Press, 1985. Pp. xiii + 193. $24.00.)

Not too many years ago a book on democracy in Latin America would have met with little interest but, with the recent democratization of the region, we can expect that this volume by John Peeler is a harbinger of a wave of new studies that will inundate the field.

If anything this is a provocative work. Peeler claims to be representative of the "post-behavioral" trend in political science that rejects the notion that research can or should be value free. He unabashedly promotes liberal democracy as "a good thing" while at the same time condemns it as a sham that stymies the search for true social, economic, and political justice. This position would of course be contradictory if not placed against the background of Latin America's most recent history. Liberal democracy with all its warts, according to Peeler, is preferable to regimes that torture, imprison, and murder their citizens. However, in attempting to go further by suggesting how, once liberal democracies are firmly implanted, Latin American polities can further democratize—creating truly equalitarian, participative democracies—Peeler fails to find a formula for this transformation and is left with the lesser of two evils.

Rare to analysis of Latin American politics, Peeler commendably begins with a discussion of liberal democratic theory and, following the works of C.B. Macpherson, concludes that "liberal democracy is essentially a liberal political system legitimated by the appearance of democracy" (p. 6). In essence, although he believes that liberal political freedoms and guarantees are essential to democratic political life, liberalism—through its defense of property and the inequality resulting therefrom—makes real democracy impossible. This said, however, it seems to be Peeler's belief that liberal democracy is at least a starting point for the creation of real democracy in Latin America. The bulk of the work then is an attempt to show how stable liberal democratic regimes, few that they are, emerge in Latin America, how they might be maintained, and how they may be transcended to create real participative democracies.

Peeler tackles each of these questions with varying success. Briefly examining and rejecting (most often without explanation) the theories of Samuel Huntington, Guillermo O'Donnell, Howard Wiarda, and Goran Therborn in which socioeconomic factors predominate, Peeler finds through a survey of the political histories of Colombia, Costa Rica, and Venezuela that liberal democracy comes about primarily as a result of elite accommodation. Elites, for one reason or the other (usually it seems because they are threatened with annihilation from below if they continue to fight among themselves) decide to live and let live. This "political act" of accommodation, while limited and conditioned by both internal socioeconomic and external geopolitical and economic forces, is not determined by them. We are thus left with chance, luck, and fortune. Economic, political, or social conflicts that violently split elites dissolve simply because they will it!

Once liberal democratic regimes emerge, the task then is to preserve them. For Peeler, this is accomplished by a combination of the same elite accommodation and the very form of liberal democracy. The consensus reached by the elites creates a hegemony around which the political debate within the country revolves. Alternatives to this hegemony are eliminated or marginalized while the veneer of democracy—free, honest elections, the right to organize, etc.—evidenced in sometimes strenuous competition for office by elite fractions, masks the domination of the system by the ruling class. With both the right and left politically marginalized, extreme economic, social, and political inequality can be maintained by leaving the voters no alternative but what is constituted as the center—the status quo interest of the elites: that is, the defense of their property and privilege.

Because in Peeler's opinion any kind of attack on liberal democracy (especially attacks from the left) would most likely lead to right-wing military coups, he urges the left to "work within the system"—that is, accept liberal democracy. This of course means that it should abandon any attempt to transform fundamentally the system of inequality and, following that, any transformation to real participative democracy. This produces the conundrum in which Peeler enmeshes himself. On the one hand, he argues that "we should work towards those ends [referring to justice and revolution]" (p. 154), but on the other hand, we cannot rock the boat for fear of a repressive right-wing coup. Although he presents several scenarios for openings to real democratic transformation, none are realistic given his commitment to the maintenance of liberal democracy.

DOUGLAS FRIEDMAN

College of Charleston

Human Rights in the Soviet Union. By Albert Szymanski. (London: Zed Books, 1984. Pp. viii + 338. $30.95, cloth; $12.95, paper.)

Szymanski describes himself as a Marxist writing on socialism vs. capitalism. Socialism wins. Szymanski has obvious sympathy for, and appreciation of, the domestic policies and achievements of the Soviet Union. He accentuates the positive.

Szymanski uses the idea of human rights, but makes no attempt to demonstrate that concern for them influences Soviet policy. To demonstrate this would be difficult. The evidence is that the Soviet leaders concern themselves with what they think of as interests of the people, claiming to know those interests better than the people do themselves. In promoting them, the leaders incidentally promote the enjoyment of some rights, and thus they make Szymanski proud. He cites the provision of the constitution indicating that when rights conflict with the official conception of interests, rights must give way, but he pays no attention to the implication that individuals (and whole classes) may be sacrificed when the interests of the people can thus be served.

Szymanski gives good marks to the Soviet Union in each of the areas with which he deals. In doing so he maintains a tone of scholarly reasonableness and musters substantial supporting evidence. Friends and critics of the Soviet Union should all be aware of his arguments. He does least well in assessing Soviet policies on emigration. It is enough for him that those policies are more liberal than the policies of most states have been through history. He does not mention the fact that the Soviet government has explicitly committed itself to the right of everyone to leave any country, including his own.

Szymanski's general theme with respect to minority nationalities is that Soviet policies are benign, respectful of distinctive cultures and contributing to both cultural and material advancement. The consequence is that separatist or anti-Soviet feeling is limited, and where it exists it is based on nationalist traditions rather than on any contemporary oppression by the Russians. The theme with respect to women is that the Soviet government has advanced them into at least as good a status as they enjoy in any other country. With respect to the economy the theme is that despite problems, especially in agriculture, substantial growth in production has occurred and standards of living have been rising.

Szymanski approaches the problem of toleration and repression somewhat differently. In effect he grants that the Soviet record is spotty, and he seeks to explain the fact "in materialist terms" (p. 205). His argument is that in certain kinds of circumstances governments of all sorts become intolerant and repressive, and to the extent that this happened in the Soviet Union it was on the basis of a kind of universal law and not because of anything for which a party, a person, or Marxism itself can be blamed. Anyway, he claims the number of deaths and ruined lives associated with collectivization, the purges, and so on, is usually greatly exaggerated. Since the mid-1950s "the range of political tolerance has continually increased" (p. 270). There are some dissidents, but they are few in number and have been able to generate little sympathy or support among the people.

A chapter on "The Land of the Free" goes through the history of denials of civil liberties, especially free speech, in the United States. The record is troubling, but it is important that the United States champions free speech in both its law and its ideology. The Soviet Union does not, giving priority to alleged interests of the people and to the cause of socialism as interpreted by the Communist Party. Crediting the Communist Party with serving the interests of the people, Szymanski pays little attention to the question whether it is responsive to their will; and he is silent on the question of the extent to which the party renders itself accountable to them.

It is interesting to contrast Szymanski's book with Richard Pipes, *Survival Is Not Enough* (New York: Simon and Schuster, 1984). The two authors disagree more in their selection of subject matter and in their interpretations than in their facts. Pipes accentuates the negative. Those teaching courses on the Soviet Union might find it interesting to assign both books, for they provide antidotes to each other and illustrate that scholarship is not always uninfluenced by the ideological predispositions of the scholar.

VERNON VAN DYKE

University of Iowa

Big Structures, Large Processes, Huge Comparisons. By Charles Tilly. (New York: Russell Sage Foundation, 1984. Pp. xii + 176. $14.50.)

Tilly asks how social scientists can improve their understanding of those large-scale structures (e.g., systems of states) and processes (e.g., capitalism and the formation of powerful national states) that transformed the world of the nineteenth century and continue doing so today. On the negative side, he answers that social scientists must abandon eight pernicious myths of nineteenth century thought that still encumber twentieth century social science. On the positive side, he proposes two classificatory schemata, one for distinguishing the analysis of structures and processes at four historical levels and another for comparing big structures and large processes.

The eight pernicious myths of nineteenth century folk wisdom are: (1) *Society* is a distinct entity; (2) society affects individual minds which, in turn, produce social behavior; (3) there is a general phenomenon known as social change; (4) large-scale social change takes distinct societies through evolutionary stages; (5) the mainspring of large-scale social change is *differentiation*; (6) *social order* reflects a balance between differentiation and *integration* (or control); disorder results from rapid or excessive differentiation; (7) the strains of excessive social change produce crime, suicide, rebellion, and the like; and (8) legitimate forms of conflict, coercion, and expropriation flow from the processes of integration and control—illegitimate forms flow from change and disorder.

If we grant that the term society is a reification of multitudes of structures and processes that are seldom, if ever, bounded in time, space, and personnel, then most of Tilly's other myths fall as well. Thus, if society is a fictitious entity or communal ghost, it cannot be the ultimate source of social behavior. Social change must be disaggregated into such processes as urbanization, industrialization, and bureaucratization, and there can be no general "stages" of a society's evolution. Similarly, if there is no coherent, aggregated process called social change, then differentiation cannot be the mainspring of social change and the juxtaposition of differentiation and integration fails accordingly. Finally, the notion that certain social facts (e.g., suicides and crime) result from the strains of social change fails because there no longer is a coherent process of social change, and the distinction between legitimate and illegitimate forms of force becomes impractical and obfuscating.

Tilly complements his exposure of the eight pernicious myths with a call for concrete (real times, places, and people) and historical (limited in scope to eras bounded by the playing out of well-defined processes) analyses of big structures and large processes. He cites four historical levels of analysis: (1) the world-historical, comparing the different major eras of human history; (2) the world-systemic, analyzing the contours and operations of each major era; (3) the macrohistorical, accounting for big structures and large processes and their alternate forms within a given era; and (4) the microhistorical, tracing the encounters between individual and groups and these big structures and large processes. Tilly's preferences lie with the macrohistorical and microhistorical.

Tilly also examines four methods of comparing big structures and large processes. He distinguishes "share of all instances" (getting the characteristics of one or many cases historically right) and "multiplicity of forms" (whether a phenomenon has one or many forms). From these two dimensions derive four forms of comparative inquiry: (1) individualizing, where each case is treated as unique; (2) universalizing, where properties common to all instances of the phenomenon are identified; (3) variation-finding, or the search for a pattern of variation through systematic examination of differences among all instances of a phenomenon; and (4) encompassing, or explaining different instances in terms of their location within and relation to the system as a whole. The four forms of classification stem not from the logic of comparison, per se, but from the relation of observation and theory or the questions being asked. Which of the four approaches are better for discovering "truth" depends, according to Tilly, on the actual (even if unknown) structure of the world being explained.

This book deserves the careful attention of political scientists. To be sure, as Tilly admits, he exaggerates the unity of nineteenth century thought and by so doing exaggerates its stranglehold on modern social science. Still, a core truth remains, as anyone troubled by the careless use of abstract concepts (e.g., society, differentiation, and social change) would agree. Tilly's preference for the macrohistorical and microhistorical surely is his choice, just as preferences for world-historical or world-systemic may be the legitimate choices of other scholars. More disconcerting, however, is the notion that history occupies an epistemological plane such that "sociologists, anthropologists, political scientists, and an occasional economist have begun to work at getting the history right before generalizing, in order to be able to generalize soundly" (p. 79). Alas, I find "right" and "wrong" history as limited as the distinction between legitimate and illegitimate force.

I perhaps am most troubled by the typology of comparative inquiry. Tilly readily admits that the classification schema depends neither upon the internal logic of comparison nor upon the existential referents being compared. The typology stems instead from what various researchers are attempting to do. This caveat will of course not prevent some from inappropriately using the typology as a guide to comparative inquiry. More immediate, however, is the bet (p. 147) that all four varieties will have their place in comparative inquiry and that encompassing comparison will come into its own. Here Tilly too appears to use the schema as a logic. We may well face the challenge of integrating big structures, large processes, and huge comparisons into history, but our success ultimately will rest on an equally systematic attention to a philosophy of the social sciences.

STEVEN THOMAS SEITZ

University of Illinois

First Among Equals: Prime Ministers in Westminster Systems. By Patrick Weller. (Sydney: George Allen & Unwin Australia, 1985. Pp. ix + 228. $25.00.)

Patrick Weller seeks to contribute to the sizeable collection of studies of executive power with a comparative study within the general institutional framework of cabinet government. Considering the United Kingdom, Canada, Australia, and New Zealand, he makes his overall question the power of the prime minister, defined "as the ability of prime ministers to have an impact on the intended actions of governments" (p. 180).

Wisely following the lead of Anthony King, Weller breaks down the global question into several different factors and analyzes the institutional constraints on prime ministers in each of his selected nations. Thus he describes the relationships of parties and party leaders; their selection and vulnerability; their patronage; cabinet relationships; personal advisory supports; parliamentary relationships; and roles as public figures.

Weller hopes that by comparing these verbal descriptions—and intelligent estimates of who has power and what are the constraints—one can distill out useful advice on the operation of the office. For example, if Tony Benn urges that cabinets be elected by the parliamentary party, one can look at the experience of the Australian and New Zealand Labor/our Parties and find "evidence" concerning these arrangements. Incidentally, he decides it does not make that much difference, as PMs must see various factions represented in Cabinet, cannot avoid certain leaders, and will neutralize enemies with responsibility.

The studies are intelligent and compact institutional descriptions and verbal assessments, with limited generalizations cautiously drawn from the mass of activity which these four countries represent. One can learn of party organization and rules, cabinet practice and traditions, and personal reactions of observers and participants in each of his nations.

In the end, Weller attempts the impossible and does it as well as anyone could: he rates prime ministerial power in each country and each major party on seven representative and important variables. Very tentatively, he suggests British Conservative leaders are most powerful, and the Labor/our Party leaders of Australia and New Zealand least. He immediately qualifies, and notes that no one set rates highest or lowest on all variables, and that certain individuals could make better use of their available leverage than others. After reviewing the institutions, he refuses to attempt to draft a prime minister's "job description," but argues that some institutional relationships aid a prime minister better than others.

Weller's judgments must necessarily be subjective and controversial, although he qualifies and states with intelligent caution. He tends to come down against the Crossman thesis on British ministers and notes wryly that each nation's observers appear to believe their prime ministers most powerful. While he notes the enormity of the task of ferreting out the many variables in four similar but different sets of institutions, he glosses over the fundamental problem of all literature dealing with power comparisons: that power is, as he says, transactional. Generalization and quantification must be, in any precise sense, impossible. As a student of comparative politics, this reviewer likes comparative analyses, but he must also admit it compounds the problem geometrically. But the questions are there, and Weller does a pithy job of dealing with them. He will be cited on this subject for some time to come.

One final note: Weller sets out looking for, primarily, the institutional constraints of his subjects' powers. One wonders whether his answers would be the same if he had started with cabinets, or individual departmental ministers, and listed the constraints on them.

JOHN H. MILLETT

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State Politics in Contemporary India: Crisis or Continuity? Edited by John R. Wood. (Boulder: Westview, 1985. Pp. xiii + 257. $15.00.)

The domestic politics of India, both central and state, engaged the devoted attention of a cohort of able, young, North American political scientists in the 1960s. The focus of the discipline has shifted since then. The political development framework they used has come under attack by political economists, Public Law 480 funds have dried up, and access to India has been severely reduced by the restrictive visa policies of its government. Significantly, there are no case studies of the Punjab, Assam, or Kashmir in this volume. Yet some of those who dedicated their professional lives to this specialty have persisted with increasing sophistication and the present work is therefore the best on the topic in almost 20 years. Whether it will receive the attention it deserves from fellow political scientists, however, is doubtful because the authors assume substantial background knowledge of India in their readers, as was appropriate for the Asian Studies meeting to which the papers were originally addressed, but which will inhibit comparisons of India with other democratic federal systems.

Unlike many such collections, *State Politics* does nevertheless have a theme and a set of questions each contributor is asked to answer. The theme is the growing importance, yet also increasing diversity, of state politics in all-India governance. The main question is whether the 1980 election (and, despite Indira Gandhi's departure from the scene, one might now add the 1985 poll) marks a return to normality in the "Congress system" or whether political decay in the form of de-institutionalization has gone too far to avoid a revolutionary crisis. All contributors opt against the latter possibility, at least in the short run.

All of them (Paul Brass on Uttar Pradesh, Harry Blair on Bihar, Atul Kohli on West Bengal, Ouseph Varkey on Kerala, James Manor on Karnataka, Jayant Lele on Maharashtra, John Wood on Gujarat, and Roderick Church as discussant) also focus on the struggle of the "backward" lower (but not untouchable) castes against the "forward" upper castes for their share of the political and status spoils of the system. Each has to struggle with how to assign the significant sub-castes or *jati* to both the Hindu ritual hierarchy of the *varnas* or castes and to economic classes since, except for the top (Brahmins) and the bottom (Untouchables), these do not cut across regional boundaries. In general, their "Forwards" are not Brahmins or big landlords, but dominant middle-peasant sub-castes, ominously termed by Brass and Blair as Kulaks, farmers who employ landless laborers. The "Backwards," who now demand reserved quotas like those already accorded the Untouchables and Tribals, are poor peasants or artisan sub-castes. Most of the authors recognize the poor fit between caste and class because of large variations in economic status within each jati and consequent overlap. Kohli is the weakest in this respect as he relies on the Bengali category of the Bhadralok (respectable people) which is an educational and occupational term, without explaining its sub-caste composition. It is hard to accept his assertion that caste is weakest in Bengal. Lele resorts to the term elites without making it clear whether these are positionally or ascriptively defined.

Blair is the only contributor who gives the Political Economy approach any more than token application, and his work is the most statistically sophisticated. Varkey has the least economic background data. But all the writers share an unthinking liberal use of the terms "progressive" and "reactionary." The question is only whether growing politicization and participation by the Backwards in an economy of scarcity has increased corruption and violence to the point where democracy is in jeopardy. Implicitly their sympathies are with the downtrodden, except for Brass who gives a favorable account of Charan Singh's agricultural program for the rich and middle peasants. There is short shrift for the relative deprivation of the "Twiceborn" varnas whose natural resistance to downward mobility has been increasingly violent and manipulative at the price of noblesse oblige. None of the studies except the editor's on Gujerat deals with "Sanskritization," the ritual mobility of lower sub-castes through adopting the values and lifestyle of the upper. He shows how Rajputs in Gujarat extended their Kshatriya (warrior) status downwards in order to increase their vote banks.

THEODORE P. WRIGHT, JR.

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Industrial Policy: Business and Politics in the United States and France. Edited by Sharon Zukin. (New York: Praeger Publishers, 1985. Pp. xi + 256. $36.95.)

This volume is based primarily on papers given at a conference on state policy and business strategy in the United States and France in October, 1983. The result is 21 separate chapters, all of them relatively short, and each one gives a substantive overview on some aspect of the topic. The chapter authors are a fairly heterogeneous lot, about half being full-time academics while the rest, some of them authors, are active in organizations directly involved in industrial planning for the two countries.

To the editor, the core of industrial policy is reindustrialization which involves a state-led effort to reconstruct a nation's industrial base, something which cannot be done by market forces alone. This process demands the channeling of investment capital to industries that have realistic prospects for expansion given a country's resources. On the other hand, industries on the decline demand rationalization, deinvestment, retraining of workers, and the introduction of new industries into regions most hard hit by the presence of declining industries. The state is to lead the way with subsidies, tax expenditures, and regional and export programs that will bring about the necessary national economic transformation.

Does a comparison of business and politics in the United States and France shed any light on what is effective industrial policy? The editor argues yes because state-centered France represents a proper contrast with the market-oriented United States. The contrast, however, turns out to be more one of rhetoric; that is, from the American side there is a debunking of the tools of French industrial policy. Yet when the United States takes hesitant and small steps toward an industrial policy, the basic shape of the implementing ideas is virtually identical.

There are many tools of French industrial policy, but among the most interesting is the idea of a *filiere* or production channel. Christian Stoffaes, director of the Office for Computer and Data-Processing Industries in the French Ministry of Research and Industry, explains this as a strategy of a nation to control the chain of production within an industry from the basic raw materials to the final consumer product. Such a strategy recognizes the synergies among firms in an industry especially between parts suppliers and industrial consumers and among the main manufacturers themselves. In the French case, the *filiere electronique* or electronics complex seems to be especially successful in the 1980s.

While this volume is very useful to beginning students of industrial policy and those who want to discover the basic facts and current trends of industrial policy in the United States and France, there is little new for political scientists with a rudimentary background in these areas. The main reason for this is that the authors cover the conventional wisdom in a general way. There is no new research reported on here and 9 of the chapters have neither notes nor bibliographic references.

Substantively, a noteworthy gap exists in both the literature and this volume that concerns the interaction of the state and business over the public policy development for industry—that is, the role of business interest associations (BIA's). For all too many authors and participants in this policy process the significant players are individual business firms and the state. The BIA's are ignored, an unfortunate omission since there is good reason to believe that they can and do provide effective leadership in industrial policy. For example, industry-wide associations can be very useful in research and development by eliminating duplication of efforts by firms, by obtaining economies of scale, and by quickly communicating this knowledge to individual firms in an industry.

Finally, there are several ways the volume could have been improved. The introductory chapter should have been the basis of a concluding one which built upon the essays of the contributing authors. The chapters should have been fewer in number, longer, and written by academic scholars. These chapters could have been introduced by the editor who then could explain to the reader why the chapter topics and authors were chosen. Conferences can result in useful anthologies such as this one, but with revisions, paring down of contributors, and extensive editorial work, such volumes may become enduring aids to academic scholars.

HENRY J. JACEK

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INTERNATIONAL RELATIONS

Regionalism and Global Security. Edited by Gavin Boyd. (Lexington, Mass.: Lexington Books, 1984. Pp. vii + 194. $25.00.)

Gavin Boyd has edited and contributed much to a fascinating and timely book. There is little doubt that the present world is quite different from and more complex than the raw power-centered world many expected (and hoped!) would occur after 1945. Political science practitioners have made major progress in catching up to reality and now talk the language of interdependency. Today many see international relations in more complex interactive terms. There is an inclination to recognize the need to discuss issues not as politics or economics, but as political economics, which is a helpful step forward. Boyd and those whom he has assembled move in a related profitable direction by hooking political and economic cooperation to a lessening of international tensions.

One of the pleasant aspects of the book is that Boyd has orchestrated a unified, yet edited, discussion. All of the essays treat common themes: the possibilities for cooperation within a variety of interdependent relationships, and, perhaps more importantly, the constraints that continue to inhibit such potentials. There is an evenness in tone and argument across each of the essays. This reviewer found them to be basically well argued and thoughtful, if not always finally persuasive. The following roster makes a worthwhile read: "Foreign Economic and Security Policies," by Gavin Boyd; "Atlantic Relations," by Phil Williams, William Wallace, and Stephen Woolcock; "African Community Building," by Sam C. Sarkesian; "Pacific Community Building," by Gavin Boyd; "Latin American Regionalism," by Gavin Boyd and Yale Ferguson; "Middle East Community Building," by Mary Ann Tetreault; and "Regional Design," by Gavin Boyd.

While I believe that the several authors are surely pointed in the proper direction, there are several points with which I have some rather deep reservations. These disagreements stem from what I perceive to be a general nonrecognition of the deep-seated structural reasons why some of the constraints function as profoundly as they do to inhibit, indeed one should say prohibit, meaningful planning for cooperation. For example, the fundamental tensions and inconsistencies present in liberal political economics at the domestic and international levels are overlooked. The liberal separation of political and economic agenda building and implementation stand directly in the way of making progress in the way that the authors desire. There is much more gravity than the several authors argue here in the liberal system of economically driven political decision making.

This characteristic of stopping short of the more convincing case is again evident in the authors' elaboration of the crucial roles played in each region by the United States and other dominant players. They document clearly that the dominant nations in the various regions generally act to constrain cooperative actions rather than to enhance the possibilities for them to occur. But the authors do not go on to the next step which would be to examine why in terms of decision-making structures what they observed occurs. This book provokes thought. The themes are relevant and I hope it helps generate the sort of meaningful discussion which the topics of regionalism and regional cooperation in the context of global security deserve.

CARL PINKELE

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National Security Strategy: Choices and Limits. Edited by Stephen J. Cimbala. (New York: Praeger Publishers, 1984. Pp. xi + 371. $34.95.)

Cimbala's text is the latest volume of the Foreign Policy Issues Series produced by the Foreign Policy Research Institute (FPRI). This is a collection of essays loosely organized around various concepts in national security strategy, which Cimbala defines as "a process for making decisions, a way of relating the national organism to its environment" (p. 1). In his brief introduction to the essays, Cimbala posits strategy as purposive, manipulative, systematic, dialectical, and political. His principal criticism of U.S. national security is that we have such little strategic concept in our national strategy. Instead, we "have substituted technology and slogans for strategically conscious policies. Our strategies have been reactive to Soviet objectives and criticisms . . . rather (than) to our own requirements" (p. 6).

The book offers no panacea. Cimbala believes that only "marginal improvements are possible" in national security policy making. The essays themselves certainly operate in the marginalia, offering few significant proposals for change—except for the five essays which address strategic forces. Those essays are provocative and controversial; they are certain to upset many readers with their frank discussion of nuclear war-fighting strategies and capabilities.

The writers are principally political scientists who, like the prevailing theme at FPRI, hold somewhat conservative views of international politics. Most hold dual credentials, in that they have served both in government and in academia. Thus, their essays reflect considerable pragmatism and are sensitive to the political/bureaucratic realities of transforming national security policies.

Nevertheless, the nuclear strategy writers are clearly dissatisfied with current nuclear strategies, even in the Reagan era; they propose the enhancement of strategy, if not of force levels. Colin S. Gray suggests that U.S. nuclear strategy is confused and that our objectives are irrelevant. Successful war fighting requires a commitment to victory, however defined, in order for deterrence to succeed. Gray advocates more active and passive defenses, survivable command and control systems, and more flexible targeting.

In a similar vein, David Tarr suggests that the U.S. shift from a policy of nuclear deterrence to one of nuclear defense. Unlike the current administration's defense policy of "more of everything," Tarr believes that more nuclear defensive systems would substitute for some nuclear offensive (deterrent) systems. Thomas A. Fabyanic also takes issue with the Reagan administration, as much for its method of making strategic decisions as for its strategy. Like other writers here, Fabyanic urges more study of the history and nature of warfare, rather than strategic analysis based upon statistical theory. This administration, like others, has used a decision-making process that "has relied less on the conceptual aspects of warfare and more on data outputs of various quantification techniques" (p. 259).

Political scientists in general probably will enjoy most the three short concluding chapters on the nature of strategic policy making within the executive branch. These are high quality analyses easily adaptable to textbooks on American government, written by seasoned scholars: Vincent Davis, Sam Sarkesian, and John Allen Williams.

The text suffers to a very small extent from the glitches that usually plague national security studies: occasional items of misinformation, dated material in a highly-perishable field of study, insufficient explanation of tables drawn from government studies without accompanying narratives, or non-standard (and unexplained) abbreviations. These are minor problems in an otherwise well-prepared text.

The greater problem is conceptual. Cimbala suggests that we have begun a new great debate on national security policies because the strategic environment has changed dramatically in recent years. He offers "a conceptual perspective" of strategy, which is then largely ignored by the writers. This is not to fault the essays, for they are generally excellent. The organizing theme, however, disappears from view and the reader is left with a 1982 snapshot of selected portions of the "new great debate." It is difficult to assemble a collection of policy essays which remain consistent to a central framework for analysis, especially when written by political scientists who are justly proud of their iconoclasms.

ABBOTT A. BRAYTON

East Tennessee State University

Citizens and Soldiers: The Dilemmas of Military Service. By Eliot A. Cohen. (Ithaca, N.Y.: Cornell University Press, 1985. Pp. vii + 227. $22.50.)

This is an outstanding analysis of America's difficulties in settling on a durable system of peacetime military service. *Citizens and Soldiers* certainly belongs on the reading list of any course on national security policy.

Cohen's book is distinctive because it excels in three areas: historical development, comparative analysis, and philosophical dimensions of public policy. Most authors would have been satisfied with highlighting the history of military service in the United States. But Cohen compares and contrasts America's experiences with those of other major as well as small nations. He also discusses military service in terms of liberalism and egalitarianism. Cohen analyzes "the difficulties that stand in the way of a sound American manpower policy: the strategic problem of having to prepare to fight two very different kinds of war; the philosophical problem of reconciling opposing notions of just obligation; and, overarching both, the practical difficulties imposed by the demands of war and normal political life" (p. 152).

One of the most interesting sections of the book is the discussion of the all-volunteer force. The author argues that the assumptions of the all-volunteer force reflected the doctrines of liberal (free-market) economists. That, combined with an unpopular war, discontent with the draft, and a short-term increase in the pool of available manpower, made the all-volunteer force politically irresistible.

What has occurred, according to Cohen, is "a determined and desperate effort on the part of Democratic and Republican administrations to prove that the all-volunteer force was working" (p. 169). The four types of criticisms of the all-volunteer force (unrepresentativeness, difficulties in maintaining numerical goals, declining quality, and problems in overall combat efficiency) are difficult to dismiss—especially since it really has not been tested in a major war.

Cohen's suggestions for improving America's manpower policy are not fully satisfactory; but, admittedly, there are no facile solutions. He favors a "universal militia draft" (p. 187). He believes that a successful military manpower policy must "(1) . . . be consistent with both liberalism and egalitarianism. In other words, service should be brief and limited, but all or almost all young men should serve. The program must have a clear military rationale. (2) The system must produce effective forces for both total and small wars: this means that the standing army must consist of volunteers. (3) The system must not be excessively costly, nor may it place excessive organizational and manpower burdens on the armed forces" (p. 186).

Cohen dismisses two alternatives to this policy: maintaining a fragile all-volunteer force or reducing it and transforming it into a professional force. However, this latter alternative seems more likely than Cohen admits—even if it would require major withdrawals from Europe and a substantial alteration of U.S. foreign policy interests.

ROBERT H. PUCKETT

Indiana State University

Power in Numbers: The Political Strategy of Revolt and Rebellion. By James DeNardo. (Princeton: Princeton University Press, 1985. Pp. xiii + 267. $25.00.)

For the past 20 years political scientists have tried to understand why people would choose, on a rational basis, to revolt against the existing political order. Early attempts to build rational choice theories of protest and rebellion encountered what seemed to be an insurmountable obstacle: the free-rider problem. Individuals bear the costs of protest, in terms of time and, frequently, government repression. Yet a single individual's participation will not noticeably affect the probability that the protest will succeed in effecting desired changes. Since a person will reap the rewards of any changes that might occur if the protest is successful, whether he participates or not, why should he participate?

In recent years, several people have tried to get around this obstacle by introducing assumptions that would mitigate or eliminate the free-rider problem. DeNardo's book is the most ambitious of these attempts. In this book the free-rider problem is eliminated by assumption. DeNardo assumes that people don't discount the benefits of a successful protest by the probability that their individual contribution changes the outcome, but rather simply consider whether their preferred policies are closer to those enacted by the government or those advocated by the protestors. In the first part of the book (dealing with peaceful protests and no repression) this is the only consideration that determines whether an individual protests or not. In later chapters, DeNardo adds to this basic mechanism the costs of government repression and the use of violence by protestors. On the government side, DeNardo assumes that leaders have a (unitary) preferred policy position but are willing to compromise on this in order to reduce either the number of people who are protesting or the level of violence.

With these basic assumptions, DeNardo then analyzes the strategy choices of protest leaders. His analysis focuses on two levels. First, what strategy will yield policies closest to those preferred by protest leaders? Secondly, if protest leaders differ over the policies they most prefer or the value of a compromise, how will factions and coalitions among protest leaders develop? DeNardo reaches interesting conclusions on both levels. He offers a persuasive argument as to why leaders who are unable to attract enough support to succeed in changing a policy to one that meets their ideal may actually secure larger concessions from the government if they either moderate their demands or, alternatively, engage in violent protests. He is also persuasive in explaining why general conclusions on the impact of moderation will not hold under more complicated scenarios that include government repression. These scenarios are important because optimal strategy becomes indeterminate, not as a matter of principle, but rather because the amount of information required to make a determination is staggering and generally unavailable both to protest leaders and outside observers. This indeterminacy is also relevant to DeNardo's analysis of factions and coalitions among different protest leaders. Although DeNardo's analysis of factions rests largely on the preferences of leaders and the responsiveness of government, one could argue that a third factor—information estimates—may also enter the picture, probably to reinforce the prior inclinations of protest leaders.

To some extent, the strengths of this book are also its weaknesses. As one example, DeNardo ignores the free-rider problem and as a result can carry out a complex analysis of rational strategy choices. On the other hand, although free-rider problems are not as pervasive as expected utility analysis would make them out to be, it does not seem reasonable that rational actors should evaluate public goods as though they were private goods. As a second example, DeNardo often adds a variety of substantive assumptions about preferences. This "fleshes out" the theory and ties it in with other work in the field. On the other hand, conclusions may then be very close to assumptions and not very different from arguments that are already well known as, for example, that repression may either increase or decrease participation because it both generates anger and imposes costs. Finally, the amount of indeterminacy in strategic choices is interesting and may serve to explain why it is so difficult to predict mobilization or outcomes of protest movements. This indeterminacy, however, is also an obstacle to empirical tests. When all is said and done, the most important contribution of this book is that it extends a rational analysis of protest beyond that accomplished by previous works. If protestors are rational (and I assume they are) that is an important contribution.

BARBARA SALERT

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A Continent Apart: The United States and Canada in World Politics. By William T. R. Fox. (Toronto: University of Toronto Press, 1985. Pp. xv + 188. $10.95.)

Canada and the United States: Enduring Friendship, Persistent Stress. Edited by Charles F. Doran and John H. Sigler. (Englewood Cliff, N.J.: Prentice-Hall, 1985. Pp. vi + 256. $15.95, cloth; $7.95, paper.)

Prompted in part by controversial economic, military, and environmental policies which strained ties between Washington and Ottawa in the 1980-83 period, the North American academic community has shown a renewed interest in contemporary Canada-U.S. relations. These volumes are but the latest in a series of publications which explore the nature of the relationship between the two neighboring nations. (See also John W. Holmes, *Life With Uncle: The Canadian-American Relationship* [Toronto: University of Toronto Press, 1981]; Stephen Clarkson, *Canada and the Reagan Challenge* [Toronto:

James Lorimer, 1982]; Kenneth M. Curtis and John E. Carroll, *Canadian-American Relations: The Promise and the Challenge* [Lexington, Mass.: Lexington Books, 1983]; Edelgard E. Mahant and Graeme S. Mount, *An Introduction to Canadian-American Relations* [Toronto: Methuen, 1984]; and Charles F. Doran, *Forgotten Partnership: U.S.-Canada Relations Today* [Baltimore: Johns Hopkins University Press, 1984].)

Americans basically take Canada for granted. In most surveys, Americans perceive Canada as being the nation which is the "most friendly" to the United States. On the other hand, they rarely reflect on how important Canada is to the United States, both strategically and economically. For example, even two of the past four U.S. presidents have publicly referred to Japan as being America's leading trade partner, when in fact trade flows between the U.S. and Canada are much larger, and the U.S. actually exports more to the province of Ontario alone than it does to Japan.

The Fox book is derived from a series of lectures which the professor emeritus from Columbia presented at the University of Toronto in 1982-83. Fox unabashedly considers North America north of the Rio Grande as a "continent apart" (pp. 6 and 144). Two of his seven chapters (chap. 1 and 4) examine the special characteristics of a "two-country North America" (p. xiv), two (chap. 2 and 3) explore within a broad historical context the involvement of other states in North American affairs, and three (chap. 5-7) look specifically at the evolution of Canada-U.S. relations and the range of policy choices available to each nation in the contemporary international arena.

The scope of the book is too limited and the discussion of current bilateral and multilateral issues too brief for this volume to serve as the principal text in college courses devoted to U.S.-Canada relations. Moreover, the author would have served his audience well by adding another chapter on the lessons to be learned from how the U.S. has interacted with Canada in comparison to how it has pursued relations with the rest of North America north of the Isthmus of Panama. On the other hand, Fox's detailed discussion of the "miraculous nonevent" (p. 33), the fact that the U.S. did not formally annex Canada, and the complex strategic challenges currently facing each nation (chap. 6) would be good supplementary material for both U.S.-Canada and basic U.S. foreign policy courses.

The Doran and Sigler edited volume was prepared under the auspices of the American Assembly and the Council on Foreign Relations. Twenty years earlier, the American Assembly had sponsored its last volume on Canada-U.S. relations under the able editorship of John Sloan Dickey. Because of the vital importance which should be attached to this bilateral relationship, and the multiplicity and complexity of outstanding issues, one hopes that we will not have to wait another two decades for the next volume in the series.

This academic endeavor is truly binational, with the U.S. academic, Doran, and his Canadian counterpart, Sigler, jointly providing a brief but insightful introduction, and a more lengthy concluding essay on change and continuity in Canada-U.S. relations over the past 20 years. Canadian university representatives Gordon Robertson, J. L. Granatstein, Richard G. Lipsey, and Albert Legault examine the bilateral relationship respectively from the vantage points of federalism, post-World War II diplomatic history, economics, and defense. U.S. professors Seymour Martin Lipset and Lynton K. Caldwell tackle the cultural and environmental issues.

On the whole, this is a very informative volume and is highly recommended as a supplementary text in courses devoted to the bilateral relationship. One minor weakness is that the authors are not certain about the nature of their audience. Several rightfully assume that the reader may not be conversant with the current literature and issues, so they provide good introductory and background material. A few of the authors, however, apparently took for granted that their audience has followed developments in the bilateral relationship on a regular basis. This is particularly the case with the Caldwell chapter and his discussion of Poplar River, the Garrison Diversion, and Richelieu-Champlain, and to a lesser extent with Lipset's cataloging of authors and publications, and Legault's periodic lapses into jargon. All three of these pieces, however, do provide some invaluable insights and lay readers should take the time to digest the material.

Lipsey refers to an "asymmetry of knowledge" (p. 69) and surveys have indeed shown conclusively that Canadians know much more about the United States than U.S. residents know about Canada. Nonetheless, it is disturbing to find that so many Canadian scholars and government officials have such generalized and sweeping notions about the policy-making process in the U.S. (see pp. 66-68, 83). Apparently, few make an honest effort to grasp the complex nature of relations between the executive and legislative branches in Washington, or the impact which federalism, bureaucratic in-fighting, and electoral pressures can have on the resolution of such pressing issues as acid rain and liberalized trade. When this tendency among certain key officials and the attentive public north of the border is combined with the general ignorance and lack of awareness about Canada which is so pervasive south of the 49th parallel, one worries at times about the ability of the two governments to cope with a very extensive and extremely complex bilateral policy agenda.

Fortunately, more attention is now being accorded to various dimensions of Canada-U.S. relations by the academic community in both countries, and the Doran-Sigler book is a noteworthy addition to the available literature.

EARL H. FRY

Brigham Young University

Detente and Confrontation: American-Soviet Relations From Nixon to Reagan. By Raymond L. Garthoff. (Washington: The Brookings Institution, 1985. Pp. xviii + 1147. $39.95, cloth; $16.95, paper.)

As the size of this book testifies, the causes of complex events are rarely quickly—or briefly—told. Raymond L. Garthoff, a distinguished Soviet specialist with both academic and foreign service experience, now a senior fellow at the Brookings Institution, has undertaken the ambitious task of chronicling the rise and—at much greater length—the collapse of detente. The book is huge, unwieldy, awkwardly organized, and ultimately, profoundly depressing. It is also a remarkable scholarly achievement: quite possibly the most important study of Soviet-American relations to have been published during the past decade.

Garthoff's sources for so recent a topic are of course limited. He depends heavily, but by no means uncritically, upon the memoirs of such key participants as Nixon, Kissinger, Ford, Carter, Vance, Brzezinski, and Haig. He has mastered the relevant secondary literature by Western scholars, as well as a surprisingly extensive array of Soviet writing on detente, not all of it as predictable as one might think. Garthoff's long career within the State Department (most recently as Ambassador to Bulgaria), has also provided him with a fair amount of inside information, which he puts to good use. And he has even taken advantage of a quirk of fate—the 1979 seizure of the American embassy in Teheran, and the Iranians' diligence in pasting back together what came out of its paper shredder—to draw upon once-classified documents detailing events prior to the Soviet invasion of Afghanistan. The result is a sophisticated multi-dimensional analysis, unmatched in breadth, depth, and thoroughness by any other treatment of the subject now in print.

It is also certain to be controversial, because of Garthoff's insistence that the United States must bear primary responsibility for the demise of detente. There was, he argues, no deliberate or sustained conspiracy to bring this about, other than the one orchestrated by the late Senator Henry Jackson, who does not come off at all well in this book. What happened, rather, was a triumph of expediency over strategy: an accumulation of shortsighted and mostly even well-intentioned blunders that had the cumulative effect of arousing suspicion, ill-will, and ultimately a deep sense of insecurity in the Kremlin. Washington's errors, Garthoff suggests, were of two kinds: good strategies wretchedly executed, and wretched strategies wretchedly executed.

Richard Nixon and Henry Kissinger fall into the first category. Garthoff has no real quarrel with what they were trying to do, but he argues—with all the enthusiasm one might expect from someone who witnessed the process from a largely-bypassed State Department— that they did it very badly. Excessive secrecy and centralization, however purposeful they may have seemed at the time, in fact caused confusion, an inability to draw upon expert advice, and ultimately such widespread cynicism as to preclude building any effective domestic base of support for detente. Garthoff accepts Kissinger's thesis that Watergate weakened the administration's hand at a critical moment, but he points out that this was an entirely self-inflicted wound. The Russians did nothing to cause it, and even regretted its effects.

But the Republicans did at least have a strategy—the Democrats, Garthoff asserts, lacked even that. Carter himself was an earnest lightweight; his secretary of state was passive and ineffectual; and his national security adviser was an irresponsible, militantly anti-Soviet alarmist, given to outbursts of paranoia about imaginary "arcs of crisis." The Carter administration botched the SALT negotiations by appearing to renege on previous commitments; it irked the Russians by stressing human rights; it chose to complete Kissinger's rapprochement with China without Kissinger's caution that such a move must never appear to be directed against the Soviet Union; it allowed itself to be distracted by unnecessary flaps over the neutron bomb, the deployment of theater nuclear forces in Europe, and the Soviet combat brigade in Cuba; and, finally, it grossly misread the sense of reluctance and defensiveness with which the Soviets moved into Afghanistan. Garthoff maintains that detente was dead by the time the Reagan administration took office. Indeed, his brief epilogue treats the current administration with surprising respect, perhaps because he expects little from it. The Russians, he notes in passing, have been similarly respectful.

Whatever one thinks of this overall argument, it deserves to be taken seriously. Garthoff is a responsible and well-informed student of Soviet-American relations. His book is carefully, even elaborately, documented. It will certainly be difficult, after reading his account, to hold to the opposite viewpoint: that it was the Russians, and the Russians alone, who torpedoed detente.

But does the evidence sustain the thesis of primary American responsibility? Garthoff himself does not wholly exculpate the Russians. They have, he points out, meddled to no very good purpose in Latin American affairs; they have sought to encourage anti-U.S. sentiment in the Middle East; they certainly did not explain very effectively their SS-20 deployment in Europe; and they badly misjudged the impression their invasion of Afghanistan would make. He also demonstrates, though, how a succession of quite unrelated American initiatives—the reconciliation with Sadat, the modernization of American strategic forces, and recognition of the People's Republic of China—might have appeared, from Moscow's perspective, as a plot to undermine detente. He even constructs a plausible explanation for Afghanistan on the grounds that the Kremlin feared growing American influence there.

Unfortunately, Garthoff makes no comparable effort to accord legitimacy to American fears, and herein lies the principal weakness of his book. International relations, after all, involves a mixture of both the rational and the irrational. No government can be entirely successful in separating one from the other; many do not even try. Garthoff blames American officials for setting off irrational concerns in the minds of Kremlin leaders, but he seems unable to acknowledge that the process could have worked the other way as well. Indeed, the American political system, with its constitutionally-mandated division of responsibility for foreign affairs, may well be more susceptible to such dislocations than its more highly-centralized Soviet counterpart. And yet, it is the United States that is faulted for not being sensitive to Russian concerns.

In a policy-related sense, it is useful to have this argument made. There is always a gap between one's intentions and the effects of one's actions, and Garthoff's book provides convincing illustrations of how this can happen. But as a balanced historical account of the collapse of detente, written with equal attention to both rational and irrational perceptions on each side, this volume, for all its impressive qualities, falls short.

JOHN LEWIS GADDIS

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Dilemmas of Government: Britain and the European Community. By F. E. C. Gregory. (Oxford: Martin Robertson, 1983. Pp. 265. $24.95.)

Fiscal Harmonization in the European Communities: National Politics and International Cooperation. By Donald J. Puchala. (London: Frances Pinter, 1984. Pp. xii + 158. $25.00.)

Professor Gregory's topic, the effects of European Community (EC) membership on the British system of government, covers an issue of particular importance for those who want to understand how the political systems of nation-states change when they join integration-oriented, supranational organizations. Unfortunately, the analysis includes some grammatical errors (p. 185) and is so poorly executed that it hinders the reader's ability to understand Gregory's point of view. His treatment of the topic from a structural and legal perspective focuses on constitutional issues (chapter 2); the further erosion of parliamentary power after entry into the EC in 1973 (chapter 3); the attempts of the executive branch (both civil servants and ministers) to cope with the EC as a major source of policy initiative (chapter 4, written by Freida Stack); the role British officials and politicians can play in EC institutions (chapter 5); and the impact of EC policies on Britain (chapter 6).

The author's potentially valuable contribution is marred by his failure to follow an adequate research design. Gregory's analysis of how Parliament has been affected by British entry into the EC illustrates this oversight. He begins with vague hypotheses regarding both the post-entry call for domestic electoral reform and the development of select committees to increase parliamentary control over EC policy. He then presents evidence which cannot test the hypotheses and must conclude that the results are still "open issues" (p. 120). Throughout the book, Gregory's remedy is to stray from the stated purpose of his research. Conclusions are replaced by prescriptions: the reader is told that ministers need to be good at bargaining in order to maximize British interests (p. 205), but is not told how British interests and ministerial behavior have been influenced by EC membership. Unsubstantiated views are aired: all governments since 1973 have adopted a negative and low-keyed attitude to EC membership (p. 2). Irrelevant themes are introduced: Gregory discusses the "mechanics of a possible withdrawal" in the concluding chapter (pp. 206-208). Furthermore, the analysis is based on sources so readily available to the public that the only utility of the book is in the author's having compiled material which others will be forced to analyze.

In sharp contrast to Gregory's work, Donald Puchala's study of the European Community's Value Added Tax (VAT) is skillfully executed because the author does adhere to an appropriate research design. Puchala presents a nonquantitative, empirical study of how the linkage between domestic politics and foreign policy affects the ability of governments to cooperate with one another internationally. He uses primary documents and open-ended interviews with participants in policy making to sketch the efforts of EC member-states and Community authorities in Brussels to harmonize turnover taxes across member-countries between 1958 and 1979. One of the more worthwhile aspects of Professor Puchala's analysis is that he has accomplished the limited purpose for which he wrote the book. According to the author, the study does not make a "systematic theoretical contribution" (p. 10). Its significance lies, instead, in his effort to give empirical meaning to a limited number of analytical concepts: politicization, political will, political capacity, and political reverberation.

In the first chapter Puchala illustrates, through an abstract discussion of the politics of European integration, the Lasswellian assumptions about politics that guided his research and that are embodied in the four concepts around which he organizes the data. The author suggests that this conceptual framework reveals a policy process which is actually a source of strength in the promotion of cooperation and integration rather than, as is generally assumed, a factor which retards unification. The remainder of the chapter clearly summarizes the objectives and themes of the book. Chapter two takes up the notion of fiscal harmonization (Puchala actually delves only into tax policies), including both a broad discussion of how it has contributed to economic integration and a brief historical and technical account of the VAT. Chapters three through six provide the empirical substance of the book. Case studies of the West German, Dutch, Italian and British experiences with the VAT during a 21 year period are succinctly and lucidly presented. The concluding chapter is an attempt by the author to summarize what lessons can be learned about international cooperation for the future of European Integration and, more importantly in the author's mind, for relations among western democracies.

Despite the high caliber of Puchala's contribution to the study of integration and cooperation, readers should be aware that his work presumes the utility of both state-centrism and liberal theory in studying the politics of western democracies. The author's acceptance of the state-centric approach means that his analysis of domestic influence on foreign policy eliminates possible linkages between the private sector and government which other approaches consider. Puchala's willingness to endorse liberal theory correlates with his acceptance of the state-centric paradigm. His praise of Western democracy for being "the most humanistically benign form of government ever devised" (p. 145) and his desire to protect and preserve it denote an uncritical acceptance of liberal theory which belies Puchala's claim that he did not intend to make a theoretical contribution to the field. There are other drawbacks in the book. First, the author does not provide enough evidence to justify conceptually distinguishing political will from political capacity. The British reaction to the Sixth VAT Directive is the most pertinent case to use in making this distinction. Yet Puchala's analysis (pp. 121-26) fails to establish when British opposition to the Directive was illustrative of political incapacity, and when it was a consequence of failed will. Puchala also does not relate his findings to the growing body of literature on domestic politics and foreign policy of which his study is a part. This failure is even more critical since his position on the circumstances under which domestic politics takes precedence over international consensus (p. 146) appears to contradict the conclusions of others whose work he cites. Finally, Puchala's attempt to extrapolate from the European case to all Western democracies (pp. 143-52) falls far short because differences in the political-economic processes of these nations preclude generalizing from the European case. These limits do not, however, detract from what Puchala has accomplished: a solid piece of empirical work which advances our understanding of international cooperation and is so clearly and provocatively written that it can and should be read by both students and scholars in the field.

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The Dividing Discipline: Hegemony and Diversity in International Theory. By K. J. Holsti. (Boston: Allen & Unwin, Inc., 1985. Pp. x + 165. $22.50.)

This essay, written by one of international relations' leading contemporary scholars, is a spirited and seductive defense of the centrality and viability of the *classical paradigm* in international theory. Starting from the indisputable premise that international theory "is in a state of disarray" (p. 1), Holsti asks whether this paradigm continues to dominate research and teaching in the field and, secondarily, whether a genuine global community of scholars has yet emerged in the discipline. He answers his principal question with an emphatic affirmative. The classical approach continues to provide "a core for both descriptive and theoretical efforts" which "can incorporate some of the insights emerging from the challenging paradigms" (p. 144). As regards the second, Holsti concludes that Anglo-American scholarship continues to exercise hegemony in the discipline, and that the parochialism among Western scholars is even greater than in years past.

For Holsti, the classical paradigm—the inheritance of somewhat disparate traditions of Hobbes, Rousseau, Kant, and Grotius—is characterized by a focus on war and peace, sovereign states as actors, and an anarchic global system. A review of the literature, he suggests, reveals that, despite extensive criticism of this paradigm, most contemporary theory remains within this tradition. There has been a good deal of "meddling" but only two genuine alternatives occur—a "Global Society" and a "Neo-Marxist" paradigm. The first, he asserts, greatly extends the agenda of scholarly concerns, de-emphasizes nation-states, and emphasizes global interdependence; the second is relatively unconcerned with the problem of war and focuses upon class-based actors. Neither of these Holsti sees as posing a significant challenge to the dominant paradigm.

In fact, Holsti has stacked the deck. His definition of the classical paradigm is so vague as to accommodate virtually any body of research that encompasses nation-states or their governments and/or that views war as a continuing problem of mankind. This allows him to dismiss much of the international relations scholarship that emerged from the ferment of the 1970s as "marginal additions or deletions" (p. 11). Yet in order to serve as a paradigm, the cluster of assumptions that scholars make about the world they are studying must be sufficiently precise to guide research and permit cumulation of knowledge. Realism, as stipulated by scholars like Morgenthau, may have performed this function in the postwar years, but it was a highly codified version of what Holsti terms the classical paradigm; in any event, its grip on the discipline weakened significantly in the 1970s.

Holsti is certainly correct in arguing that the "behavioral revolution" did not itself entail a paradigm shift; all too often concern with theory was sacrificed to methodological preoccupations during this revolution. He is also correct in asserting that the meddlers do not provide an alternative paradigm. Nevertheless, it requires a suspension of disbelief to accept his claim that scholars in areas as diverse as crisis decision making, integration, issues, regimes, and bureaucratic politics (to name a few) are merely laboring in the same old vineyards even though they are unaware of this. They may agree that war, government authorities, and the absence of system-wide authority are important factors, but the levels of analysis at which they work, the variables they consider, and the agenda of problems that preoccupy them are vastly different and more complex than those of their predecessors. To the degree that there was a classical paradigm, it implied a parsimonious model of a single set of dominant actors (black-boxed states) whose behavior was determined by system-level characteristics, notably the distribution of power. It assumed, moreover, that these states acted rationally in response to shifts in the distribution of power (national interest). In contrast, contemporary scholars have thrown open the black box, and are seeking the causes of war in the psychology of leaders and groups, societal characteristics, and so forth. In many cases, they are preoccupied by political violence *sui generis* (not merely interstate war), and their image of the "state" has only a nominal relationship with that of their classical predecessors. To suggest that they remain in the classical tradition because of their interest in war would be akin to claiming that shamen and modern physicians follow the same paradigm because of their mutual interest in disease.

To be sure, there is no definitive answer to just *how* similar scholars must be in order to be regarded as paradigm brothers, but Holsti's argument masks the degree to which the field has already flown "apart into ghettoes of specialization" (p. 144). His cross-national examination of textbook references to reinforce his claims regarding theoretical continuity and the absence of a global community of scholars is unsatisfying for several reasons. Textbooks inevitably lag behind intellectual currents and must be written in order to appeal to a generation of teachers reared in the older tradition. In addition, Holsti neither clarifies the criteria for his sample nor does he weight the references in his texts.

Although his advocacy of the classical tradition is unpersuasive, the essay is an important one because Holsti performs the critical task of pointing up the centrality of norms and values in theory construction. This is a major contribution and suggests that the debate about which he writes is less over competing paradigms in the Kuhnian sense than it is over competing norms and their impact on the way we view the world around us.

RICHARD W. MANSBACH

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Superpower: Comparing American and Soviet Foreign Policy. By Christer Jonsson. (New York and London: St. Martin's Press, Inc., 1984. Pp. vii + 248. $25.00.)

There has long been a growing cottage industry among specialists and generalists alike, to compare various aspects of power and performance between the United States and the Soviet Union (or, in Tocqueville's case, Czarist Russia). The annual *Military Balance* of the International Institute of Strategic Studies and the various studies of the Stockholm Peace Research Institute are two of many sources of comparison that are read with great care and interest.

Thus *Superpower* cannot be considered as breaking new ground, or even providing startling new insights about old ground. What it purports to do is to discuss elements of *condominium* in the two superpowers' behavior toward each other, drawing upon three cases: aid relations with the Third World; crisis management in the Middle East; and nuclear nonproliferation. The emphasis is on foreign policy behavior, although obviously the definition must of necessity be broad rather than restrictive in order for the author to make useful analytic conclusions.

Jonsson's main thesis is that the superpowers engage in mutually reactive policies, with the Soviet Union being more reactive to the United States than the other way around; although the United States also engages in "mirror-imaging" in its foreign-policy behavior. Where this is least the case is in aid relations; where this is most the case is in nuclear nonproliferation; where the issue is more muddled is in the Middle East. I doubt that most persons informed in these matters would disagree.

The author makes a distinctive point by suggesting that the superpowers' emulative behavior is primarily due to their perceptions of themselves as superpowers, and their strong desire to preserve that perception primarily in the eyes of the other—and then for the benefit of the rest of the world. Jonsson goes on to observe:

> The USSR—the junior superpower—has been more interested in condominium-like arrangements than the United States which has generally preferred unilateral action. While both superpowers may be likely to resort to condominium-like arrangements to avoid direct confrontation in a crisis, this is seen by the United States as a provisional device, by the Soviet Union as a step toward a more permanent structure. (p. 223)

The interaction of the superpowers in various combinations of cooperation/conflict is well known and understood. The desire of the Soviet Union to be treated as equal in this global co-existence is also well known. The frustrations both superpowers have encountered as they have attempted to translate their varying forms and degrees of national power into unequivocal national advantages (preferably but not exclusively at the other's expense) is only too evident. What has not emerged, as Jonsson points out, is any reincarnation of what was viewed once, referring to the nineteenth century, as a "club" of powers, not only in *realpolitik* terms more equal than the other players of international politics, but also in the sense of these powers being acknowledged, as by "right," to be the big players in the game.

In other words, the contemporary international system has not brought any conferment of legitimacy to the two superpowers' desires to be considered not only more equal than everyone else, but more equal by right as well as by might. In fact, as time goes by, neither superpower can claim any reinforcement of their ideological claims for this acknowledgement, which leaves both of them searching for ways to assert some sort of normative justification for what otherwise appears increasingly to be simply good old-fashioned national pride, ambition, perhaps even hubris, and certainly opportunism. Jonsson observes: "Although their influence may be declining, the superpowers are not likely to drastically alter their global role. Each forces the other to maintain its 'superpower role'" (p. 225).

In conclusion, it is not likely that the notion of condominium will ever have much appeal to either of the superpowers over the long haul, or to any of the other 150-odd states composing the community of nations that exists today. This is because calculations of national power, and the linkage of such calculations to prospects of national advantage, are simply too complex. A glance at the dilemma facing the United States in trying to maintain a coherent and credible policy toward South Africa, and the dilemma facing the Soviet Union in trying to maintain a coherent and credible policy toward Poland are but two examples. Whether "friends and allies," client states, or vassals, other national units simply are not willing to acknowledge total preeminence for either superpower.

This book makes good reading, laced as it is with relevant quotes and clear historical examples. Its length is also attractive and makes it useful as part of a graduate course syllabus as well as general background reading for the scholar and practitioner of foreign affairs.

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World Trade Issues: Regime, Structure, and Policy. By Young Whan Kihl and James M. Lutz. (New York: Praeger Publishers, 1985. Pp. xvi + 274. $25.95.)

Kihl and Lutz explain in their introduction that they do not attempt a comprehensive discussion of world trade, but rather seek, "to specify certain interaction patterns in the world trading system, their important consequences, and the political and economic policy responses made by the various participating countries in world trade" (p. 2). The authors also admit that their work may raise more questions than it answers, and it certainly does.

Following the introductory chapter are 11 chapters organized in four parts. "Part I: Theory and Politics of Reform" discusses, in chapter 2, what is meant by regime and five stereotypes of world trade. The authors indicate their attachments to free trade liberalism but never clarify that "real world" policies are more complex and mixed than their five stereotypes. The third chapter outlines the content and last 15 years' debates on the New International Economic Order (NIEO). Kihl and Lutz fail to integrate these two chapters, add nothing new to the literature, and ignore North-South politics so ably described elsewhere, as in Robert I. Rothstein's *Global Bargaining: UNCTAD and the Quest for a New International Economic Order*, (Princeton: Princeton University Press, 1979). Since both chapters are ahistorical and incongruous, one questions the purpose they serve.

The four chapters of "Part II: Changing Commodity Structures" do not elucidate the connections among chapters. Chapters 4 and 5 discuss exports of manufactures from developed and newly-independent countries, respectively. While the authors provide interesting statistical analysis of changes in export volume for groups of manufactures, they use 1968-78 data. Instead of tracing interacting trade patterns, Kihl and Lutz use exports to represent all trade (Wouldn't that help the U.S. deficit?) and country pairs, without sufficient discussion of interacting patterns. This omission becomes particularly burdensome in the third part of the book, in which the authors argue for regionalism in trade without reference to the overwhelming share of world trade occurring among Organization of Economic Cooperation and Development (OECD) states. Chapter 5 repeats the illusion that reference to a stereotype, in this case NICs, is an adequate discussion of policies. Why, then, is Mexico's trade position so different from that of Singapore? Chapter 6 on primary commodity exports and chapter 7 on world food exports continue the use of categories and descriptive quantitative information on exports without either asking or answering why, how, and who benefits?

"Part III: Regional Patterns and Trends" is discontinuous with Part II. The authors seem to have divided the world geographically without recalling their documentation of North-North trade in manufactures and without mention of contemporary economic manifestations of colonialism. Thus, chapter 8 covers the U.S. and the western hemisphere; chapter 9, Western Europe and Africa; chapter 10, Japan and the Asia-Pacific Basin; and chapter 11, the USSR and "centrally planned economies." The data base for Part III does not provide support for the conclusions reached. The authors rationalize using noncomparable time periods, 1961–1971 and 1971–1976, on the basis of data availability; yet, they rely on only two sources. The data base is particularly problematic in the tenth chapter, in which Kihl and Lutz want to argue the emergence of Japan in the Pacific Basin. While Japan's role in the last 10 years is obvious, that is not the time period the authors use, in spite of the publication of the book in 1985. Chapter 11 is a puzzle, also. The authors cite bilateralism and nonconvertible currencies as "two distinct trading practices" (p. 205) without acknowledging the Soviet role in the definition of the Bloc trade regime.

The authors promise that the conclusion, Part IV which is chapter 12, will highlight major points and speculate on the future of world trade. In spite of some interesting dyadic data and discussions of regionalism, the authors fail to present a convincing picture of world trade as it exists. The two middle parts of the book would make sense as separate works, each with analysis of contemporary data. As the book stands, it raises questions already and better answered elsewhere.

CONSTANCE H. COLE

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The Quest for a Just World Order. By Samuel S. Kim. (Boulder, Colo.: Westview Press, 1984. Pp. xix + 440. $40.00, cloth; $16.50, paper.)

The Quest for a Just World Order was written under the auspices of the Center of International Studies at Princeton University as part of an on-going effort of the World Order Models Project (WOMP). It is a powerful statement in what is becoming a political literary genre. Professor Kim sets forth an ambitious scheme. Not only does he call for world "order" but this state is also to be "just." Like other works in the WOMP series, a normative path is followed on a "global" scale as opposed to an international or even a world perspective. Kim spurns the value-free world in which the traditional social scientist conducts research. On the contrary, Kim argues that if the scientist is task oriented, then values are necessary in order to render solutions applicable to those global problems under observation. In this way, values become the essential basis of systemic transformation. The value-laden observation is further connected to an assumption of the existence of "human nature" which may be then modified to better the human condition and ultimately add to the development of a world order.

The general purpose of the author's proposal is aimed at "generating the political consciousness and mobilization required to challenge and transform an unjust social order rooted in violence" (p. 335). The book is divided into three neat parts. Part one provides an examination of the current state of international relations theory focusing on methodology. The particular focus is on a normative theory of world order. Part two looks at four specific problems affecting the globe. Part three contains Professor Kim's prescription for a more humane world order system that he believes is feasible.

The social organization of the world, Kim holds, has undergone systemic change over time. Man, as a social being, first focused on small groups ultimately developing to the currently acceptable order in which interdependence exists on a global scale. Since human social organization has essentially expanded to the global limits, world-order theory is not set for the benefit of any one sub-global group. Hence, on one level the need for change is evident by the pervasiveness of global problems. However, the policies enacted by many governments tend to perpetuate many of these same problems. For example, weapons development continues to escalate. There is, moreover, a growing economic disparity between the industrialized and non-industrialized sectors of the world. Also discussed is the basic threat to the world's ecological balance that recognizes no national boundary. There are, Kim argues, alternative means of achieving world order and stability.

As might be expected, the alternative world order is a cooperative venture. Together all are encouraged "to shift the course of the human journey in a more humane and just direction" (p. 308). It is hoped that as the number of individuals and groups who realize the precariousness of the global condition grow, an even larger coalition will develop to re-create the world.

SANFORD R. SILVERBURG

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La politique etrangere. By Marcel Merle. (Paris: Presses Universitaires de France, 1984. Pp. 218. FF130.)

Since Talleyrand and the Congress of Vienna, French diplomacy has acquired a reputation of being able to compensate for many internal weaknesses of the French state and society by the skill and high motivation of its diplomatic personnel, and the talent of its ministerial leaders. In France, and even more so abroad, this valuable piece of make-believe has produced an image of a stable French foreign policy detached from the various vagaries of French domestic problems.

How is it now under the conditions of global interdependence, not only in France, but all other countries? Many interesting answers are offered by a French scholar, Marcel Merle, in his most recent book entitled *Foreign Policy*. In it he sees foreign policy as only one of several segments of international relations (diagram on p. 169) that represent more than a sum total of various nations' foreign policies. The author is professor of international politics at the Sorbonne; he is also author of *Sociologie des relations internationales*, 3rd ed. (Paris: Dalloz, 1982), today the most widely used text in France; it is now also available in English.

Merle's new study is not a comparison of the contents of various national foreign policies, but a focus on, and analysis of the concept of foreign policy and the state as such. Although Merle finds the principal and traditional justification of nation state still in the duality—and difference—between foreign and domestic policy, he naturally finds that, under the conditions of interdependence, "the dikes which used to be state frontieres have been fissured and that the locks—separating the various water levels and formerly under the careful control by the diplomats—are today out of kilter" (p. 197). Under such conditions the very principle of the nation state is at stake.

Foreign policy which has expanded in terms of issues has, according to Merle, contracted in terms of its former specificity as achieved and refined in the eighteenth and nineteenth centuries. Despite the often confusing intermingling of domestic and foreign policy considerations and various non-state actors, Merle rejects the "zero-sum" concept of domestic v. foreign policy. These do not expand at the expense of each other; they intermingle. The result is not an utter confusion, but an interplay of various autonomies without the production of a homogenous bloc (which, in some cases, could indeed invite authoritarian remedies).

Merle notes (p. 93) the increasing difficulty of the central governments to know about, much less control, all the important trans-border activities initiated by noncentral governments and private actors. Even the Quai d'Orsay, now renamed by Mitterand "Ministry of External Relations," (instead of "Foreign Affairs"), once a model of tightly centralized diplomacy, had to create (May 23, 1983, p. 93) an office to monitor and coordinate the "mini-diplomacies" initiated by various regions and cities of now partly decentralized France. The author also notes the contemporary microdiplomacy of Quebec and other Canadian provinces as well as that of some Italian regions (Sardinia and Friuli Venezia) and the Walloon and Flemish communities of Belgium. Surprisingly, Merle does not take note of the mushrooming foreign activities on the part of the U.S. states (by 1985, for example, 28 of the U.S. states have established 54 permanent foreign missions in 17 foreign countries).

While Merle's book is truly comprehensive and knowledgeable about contemporary American analytical concepts, this reviewer believes that his general analysis could be usefully illuminated by references to Nye/Keohane's world paradigm and complex interdependence studies as well as Krasner's and Haas' work on international regimes.

"Is then the nation state close to its death?" (p. 201) asks Marcel Merle, noting the blurred line between domestic and foreign policy. It is, of course, an old question which had been posed, often with impatience, by liberal economists, Marxists, Saint-Simonians, utopians, and romantics. "Not at all," is his answer. Nation states—a "necessary evil" (p. 206)—will keep on managing segmentally their now globalized economies ("economies mondalisees") to make international anarchy moderately tolerable. With sad irony Merle adds: "We can count indeed on the nation state's resistance to any true international integration" (p. 119)—which logical reasoning seems to recommend so convincingly. Quoting Spinoza (p. 200), Merle warns against the dreams of a golden age or other chimeras in politics in which men will base their behavior solely on logical reasoning.

Merle's new book is a wise book whose lessons, drawn from different historical and academic environments, will usefully contribute to the current American debate on the concept of the "State."

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The Age of Vulnerability: Threats to the Nuclear Stalemate. By Michael Nacht. (Washington, D.C.: The Brookings Institution, 1985. Pp. 209. $26.95, cloth; $9.95, paper.)

Usually books from the Brookings Institution can be relied upon to help us identify the shadows on the wall of the cave more readily and to see the order with which they pass more clearly. This book is no exception. It is well written, it contains some interesting information and its conclusions remain well within conventional wisdom.

Nacht identifies what he considers the central problems of the nuclear age which, because they are ultimately unresolvable, "provide the boundaries within which effective policies should be formed and implemented" (p. 7). These problems, which are both political and technical, include: Russian national character, American national character, the new high-accuracy weapons systems, American strategic doctrine, arms control, alliances, and nuclear proliferation. Each problem is considered in a separate chapter and several of them have appeared earlier in different form as articles in various journals.

The chapters on national character are interesting but subject to the same criticisms that have been relevant to this type of analysis at least since Montesquieu. While readers will not be surprised to discover that the Russians suffer from extreme insecurity, they might be puzzled to discover that Americans are essentially schizophrenic, desiring on the one hand armaments second to none and on the other a commitment to a world "permanently at peace." While the chapter on weapons development is clear and understandable to the layperson, the one on strategic policy seems to be particularly fuzzy. We learn for instance that the first American strategic doctrine was containment (p. 85), but since that policy did not discuss nuclear weapons it was not until massive retaliation in 1954 that strategic doctrine as we know it first emerged. One is tempted to ask whether containment and massive retaliation are even comparable. Did not we have both at the same time, and in fact, is not containment still a long term goal of American foreign policy?

The final chapter is most troubling because the conclusions Nacht draws offer very little hope to those who believe that the continued development and deployment of nuclear weapons is not inevitable. When reading some of Nacht's observations one understands the feelings of Nietszche's Zarathustra who is amazed at the saint who still talks about God, apparently not having yet heard that God has died. What is one to make of a sentence which suggests that nuclear weapons "are technologically sweet . . . and they protect Americans and their allies from communist (essentially Soviet) aggression" (p. 196)? Does anyone still seriously believe that? That is a sentence one might expect to hear from Robert Oppenheimer in the late 1940s. Do nuclear weapons protect America from anything? Are nuclear weapons really weapons in any meaningful sense of the word or are they simply enormous explosions that can be linked to no rational purpose at all?

The book ends on a confusing note. The subtitle and much of the argument lead the reader to believe that some of the modern developments discussed may indeed be threats to the nuclear stalemate. After all the book looks at some of the things that it does in order to evaluate the extent to which changes were threats to the status quo. However the concluding sentence says: "It will take truly revolutionary technological innovation or a massive exercise of human stupidity before this stalemate is seriously threatened" (p. 201). If that is true, then what is the purpose of the book? If indeed there are no serious threats to the stalemate on the horizon, what accounts for the book's subtitle and the choice of subjects examined? The concluding sentence suggests that there is nothing for us to worry about, and that we can go back to sleep, provided only that neither major technological innovations nor massive stupidity occurs. In either case there is little for the intelligent layperson to do.

In short, the book seems to be an invitation to the maintenance of the status quo under the assumption that deterrence has worked in the past, will continue to work in the future, and no matter how many nuclear weapons we assemble there is nothing to fear because the stalemate is intact and nothing will go wrong. Where does this kind of naive optimism come from? Are not we long past the stage where we can leave these issues to the experts and return to our reveries? Is not the existence of enormous vertical proliferation by the two major powers the dominant fact of our day about which something must be done, even by average people, before it is too late? Considering what we now know about the fate of the earth, is it any longer enough to be reassured that the enormous arsenals on both sides "will *probably* [italics mine] prevent nuclear doom" (p. 197)? Reading this book will offer no hope out of the miasma of business as usual that characterizes so much of conventional, "sensible" thinking on this subject. If this kind of work is well-balanced and even-handed, perhaps the time has come for someone who is not well balanced to point out that the condition we are living in is absolutely insane.

PHILIP W. DYER

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Nuclear Forces in Europe: Enduring Dilemmas, Present Prospects. By Leon V. Sigal. (Washington, D.C.: The Brookings Institution, 1984. Pp. x + 181. $22.95, cloth; $8.95, paper.)

Nuclear Weapons and European Security. Edited by Robert Nurick. (New York: St. Martin's Press, 1984. Pp. ix + 142. $22.50.)

During the last decades, as the Soviet Union developed nuclear weapons capable to inflicting unacceptable damage on the United States, the American commitment to initiate nuclear war on behalf of its West European allies became increasingly incredible. This weakening of the principle of "extended deterrence," and the corresponding ambiguities in NATO's strategic doctrine of "flexible response," have led to divisive debates among NATO allies within their respective electorates and, in some cases, within their governments. Moreover, the close connection between security issues and fundamental political questions—about the nature of the Soviet threat, the future of the Western alliance, and the shape of a desirable European political and economic order—has led all parties to use the technical language of arms and arms control to express what are, at bottom, political intentions and political anxieties.

This meshing of military-strategic and political purposes was at the core of NATO's so-called "double-track" decision, the subject of Professor Sigal's book. In December, 1979, following the deployment of an increasing number of intermediate-range Soviet nuclear missiles within range of Western Europe, NATO ministers decided—this was the first track of their double-track decision—to deploy new American nuclear-armed cruise and ballistic missiles capable of striking deeply and accurately into the Soviet Union from bases in Great Britain, West Germany, Italy, Belgium, and the Netherlands. The second track of the decision was to seek arms control negotiations with the Soviet Union which would, if successful, make the first track unnecessary or reversible. The failure of the second track led to the partial implementation of the first track, leaving in its wake inflamed U.S.-Soviet relations, tensions within the alliance, and a divided public opinion in many West European countries.

In terms of extracting analytical meaning and political lessons, Professor Sigal makes the most of this apparently prototypical case of alliance mismanagement. But what makes this book eminently successful and lends plausibility to the author's political judgments is that he treats the double-track decision not as a narrowly focused case study, but places it in a much broader political, conceptual, psychological, and chronological context. Professor Sigal begins with a discussion of the paradoxes in the logic of deterrence, and the contradictions between political and strategic logic and stability, coining and applying the term "nuclear inter-dependence" as an alternative to the term "strategic parity," which he considers misleading since its connotation of numerical equality is not essential for mutual vulnerability. This leads to a discussion of NATO's public, as well as unstated rationales for augmenting its euromissile arsenal, and of the technical characteristics of ground-launched cruise missiles and Pershing II missiles which Professor Sigal regards (in my view correctly) as fitting these rationales only loosely. The core of the book, Chapter 4, examines the political dimensions of the euromissile issue. Here, as in other chapters, Professor Sigal insistently demonstrates that beyond the numbers game of calibrating the military balance on a variety of levels and rungs on the ladder of escalation, there is a central political purpose that the contending parties often leave unarticulated and in some cases even unexamined. The book concludes with a tentative and necessarily inconclusive discussion of the problems and prospects of arms control negotiations and their implications for East-West relations and the coherence of the Western alliance. Professor Sigal has given us an excellent account of the euromissile issue, and one hopes that he will turn, in the future, to a similar analysis of the Reagan administration's Strategic Defense Initiative—an issue that is even more divisive and will concern us for years to come.

The volume edited by Robert Nurick consists of chapters on the future of Britain's deterrence force (by Peter Nailor and Jonathan Alford); nuclear weapons in Europe (by Gregory Traverton); extended deterrence and U.S. strategic forces (by A. H. Cordesman); a call for "inflexible response" on the part of NATO (by Francois de Rose); and TNF modernization (by Christopher Makins). These are good, but dated and rather technical discussions for the think-tank initiates who presumably read these essays when they first appeared in various journals several years back.

WOLFRAM F. HANRIEDER

University of California
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The United States and Israel: Influence in the Special Relationship. By Bernard Reich. (New York: Praeger Publishers, 1984. Pp. xiii + 236. $29.95, cloth; $13.95, paper.)

The relationship between Israel and the U.S. is particularly complex and marked by contradictory objectives and perspectives. On the one hand, every U.S. President since Truman has affirmed a commitment and moral obligation to the independence and survival of Israel. Simultaneously, however, the U.S. has sought to minimize Soviet influence among the Arab states, many of which have had difficulty accepting Israel's presence. As a result, U.S. policy vis-a-vis Israel has often seemed to be inconsistent and unpredictable.

Largely descriptive, this volume provides a thorough review of U.S.-Israeli relations during the Carter and Reagan administrations. The negotiations between the U.S. and Israel which, no less than those between Egypt and Israel, culminated in the Camp David Accords, are chronicled in detail. In addition, the efforts of the Reagan administration in this area, including the promulgation of the Reagan Plan and sponsorship of the abortive Israeli-Lebanese negotiations in 1983, are traced.

This volume is included in a series entitled, "Studies of Influence in International Relations," and this series provides the analytic framework for the study. Influence is defined as the ability of one state to bring about changes in the policies or actions of the other. This is usually brought about by a combination of threats and inducements and is asymmetrically dominated by the stronger state. In this case, however, what appears to be almost complete Israeli dependence on the U.S. has often not enabled the "patron" to manipulate or control the "client." Occasional differences with regard to policy have led to U.S. arms embargoes and policy reassessments, but such actions have not produced desired changes in Israeli policies. "Israel . . . took action . . . where it believed its national security interest was at stake, even when it understood this would lead to clashes with the United States. The U.S. could react, but could not alter the Israeli decisions. On less central issues . . . Israel deferred to U.S. entreaties" (p. 138). For example, various U.S. embargoes on weapons sales and deliveries had little impact on Israeli policy, but did contribute to the Israeli effort to develop an indigenous arms industry. Similarly, pressure from Washington during the Carter period "had a counterproductive effect as support for Begin grew at home and abroad, partly in response to this perceived strategy" (p. 49).

Only when this strategy was changed, and inducements were substituted for threats, was Carter able to convince Israel to take the enormous risks necessary for the successful conclusion of the Camp David negotiations. In general, over the past three decades, the U.S. has been most successful in influencing Israel "through entreaties and reassurances." Israel, in turn, has been able to influence U.S. policies not through bluster and angry defiance, but by "persuasion and argument."

GERALD M. STEINBERG

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In Search of Namibian Independence: The Limitations of the United Nations. By Geisa Maria Rocha. (Boulder, Colo.: Westview Press, 1984. Pp. xi + 192. $16.95, paper.)

In terms of longevity, if not of complexity, the issue of the international status of Namibia—called South West Africa by the United Nations until 1968—has been on the agenda of the United Nations longer than any other single issue. As of 1981, consideration of its status generated 130 resolutions, 18 of which were passed by the United Nations Security Council and the remaining 112 by the General Assembly. The political, legal, and now military battle for its independence from South Africa has continued unabated for 40 years. This territory has attracted substantial international attention and has provided ample opportunity for social scientists, journalists, international legal experts, military strategists, humanitarians, and political activists of most every stripe to exercise their talents. But to what avail?

Geisa M. Rocha, now a doctoral student in political science at the Graduate School at the City University of New York, had six years experience as a civil servant working with the late Marcio Rego-Monteiro, the Secretary of the United Nations Council for Namibia. This U.N. background is reflected not so much in explicitly personal judgments as in the brevity and crispness of her treatment of the main elements of the issue. The Namibian affair, because of its multiplex nature, can become a data swamp for the unwary. But not for her.

As the subtitle of her book suggests, her inquiry deals with the inability of the United Nations organization to effect the independence of Namibia. Thus she has written a case study of United Nation's behavior. In doing so, she has provided cold comfort to those who entertain great, and unwarranted, expectations about how effective this international body can be in terms of conflict management. In the introductory chapter, she furnishes a thumbnail sketch of the origins and early practices of the United Nations with particular reference to conflicts which engage the attention and material interests of one or more of the great powers. She is not especially sanguine about U.N. resolution of conflicts that involve the vested global interests of one or more major powers.

She contends that the United Nations is not an international actor and that Western pressure upon South Africa to relinquish the territory, rather than U.N. action, is the way to achieve Namibian independence. Although she does not assess the utility of the guerrilla war there in terms of displacing the South African regime, presumably she would find it less effective than Western coercion. Yet most of her criticism is directed at non-Western states. Like other scholars, she contends that the U.N. should not have transformed an essentially political into a legal issue by frequently consulting the World Court. In addition, she avers that the General Assembly majority lost touch with reality and added nothing by passing impractical resolution after resolution, thus lowering its credibility. She sees the utility of including both the Angolan MPLA and UNITA in an independence package and comments adversely on the spendthrift nature of the "missions of consultation" undertaken by the U.N. Council for Namibia.

What is especially intriguing are her views on the spillover effects of the "loss" of Namibia on the white South African power structure. Had she chosen to do a parallel analysis of the last few years of the international politics of Zimbabwean independence, it would have added a great deal to the breadth of her study on what the U.N. is ill-equipped to do, especially in violent decolonization. The publisher would have been well advised to have required the author to have included maps and a chronology for the novice reader as well as to have proofread it more carefully. The select bibliography had no pagination for journal articles and was marred by typographical errors. Nevertheless, this is a useful contribution that challenges conventional wisdom, political inertia, and bureaucratic boondoggling.

RICHARD DALE

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Anti-Americanism in the Third World: Implications for U.S. Foreign Policy. Edited by Alvin Z. Rubinstein and Donald E. Smith. (New York: Praeger Publishers, 1985. Pp. xii + 267. $37.95.)

The essential myth of American foreign policy has been that the United States is a force for good in world affairs. This self-perception has sometimes made it difficult for Americans who expect gratitude but often receive hostility to appreciate the sources and implications of anti-Americanism. Popular explanations have pointed either to Communist influence or American malevolence and stupidity as the cause of anti-Americanism.

As a first attempt to subject anti-Americanism to scholarly investigation, this volume establishes a new level of realistic discourse on the subject. The first of 12 essays originally prepared for a March, 1984 conference is the editors' suggested approach to the subject. The next eight chapters examine anti-Americanism in specific countries and regions. The final three essays address "functional" questions: What is the relationship between multinational corporations and anti-Americanism? Does a particularly troubling type of anti-Americanism prevail within the United Nations Organization? What are the broad implications of anti-Americanism for U.S. foreign policy? Most, but unfortunately not all, of the individual studies clearly follow the editors' suggested approach.

Two major themes inform their approach. The first consists of aspects of Third World societies—ethnicity, elite values, and political systems. The second is disagreement between the U.S. and Third World states about general orientations toward world affairs and specific issues. Both imply that anti-Americanism is a highly variable concept that depends on the particular country in question and its relations with the U.S., e.g. the attitudes of Latin American intellectual elites are quite different from those of radical Moslems. Therefore, the most satisfying contributions are those with the most specific focus, such as George Grayson's essay on Mexico, Robert C. Horn's on Malaysia, and Ainslee Embree's on South Asia. Broader selections such as those on Latin America, Africa, and the Afro-Arab countries, while interesting and instructive, carry fewer useful implications for policy.

Despite the difficulty of generalizing about anti-Americanism throughout the Third World, several interesting points do emerge from this collection. First, as described in Ahmet Evin's chapter on Turkey and Harvey Glickman's discussion on Africa, the United States has contributed to its tarnished image by failing to meet the expectations raised by American can-do rhetoric, for example in the area of economic assistance. The Soviet Union may suffer less criticism because it has avoided raising such expectations.

Secondly, anti-Americanism is often found concentrated within elites and is frequently based on areas of disagreement or conflicts of interest or on the "Ugly American" attitudes and behavior of diplomats and others. Thirdly, anti-Americanism has great instrumental value. Governments often find that the United States is a useful scapegoat to absorb the blame for their economic, political, diplomatic, or military failures.

Anti-Americanism can play a useful role for a nation trying to define its identity and place in the international order. The image of a secular, consumerist America with a foreign policy of global intervention is much like the image of decadent, war-ridden, monarchical Europe that helped the young American republic to confirm its identity and its determination to become what Europe was not. It is also a useful unifying theme. It can provide a common frame of reference for discussion among intellectuals, and it can supply the basis for a minimum level of consensus among the members of the non-aligned movement.

Finally, in discussing the implications of anti-Americanism for U.S. foreign policy, Richard E. Bissell argues that, although anti-Americanism cannot be eliminated, reasonable improvements in the quality of American diplomacy and foreign policy could dampen the intensity of anti-American opinion and reinforce the reservoir of pro-American opinion. Therefore, policy makers should be more aware of the impact of U.S. policy on foreign elite and mass opinion.

The editors correctly call this volume a first cut. Several of the authors confess that their contributions are impressionistic reflections on anti-Americanism in various parts of the Third World. What is needed now is for analysts to make even more careful and rigorous investigations of anti-Americanism in specific countries.

BRIAN E. KLUNK

Union College

Foreign Policy Decision Making: Perception, Cognition, and Artificial Intelligence. Edited by Donald A. Sylvan and Steve Chan. (New York: Praeger Publishers, 1984. Pp. xi + 347. $28.95.)

This book is the first volume in an annual series, "New Dimensions in International Studies," which will attempt to document the evolution of the field by publishing collections of writings composed largely of papers presented at the annual conventions of the International Studies Association (Sylvan and Chan were co-chairs of the 1983 ISA convention). Describing in a coherent way the development of a disjointed, multifaceted, and relatively non-cumulative field like international relations would be difficult enough under most conditions, but to try to do so with the rough and tentative work represented in most convention papers seems next to impossible.

The volume does not adequately cover the central dimensions of its topic, foreign policy decision making. Even accepting the focus on recent writings with a psychological orientation, would the three main categories be "Perception and Personality," "Cognition and Learning," and "Artificial Intelligence" (the three parts of the book)? The co-editors provide a nice overview of the field in the first chapter, but it does not logically lead to the book's organization; indeed one of the four key substantive elements in Sylvan and Chan's foreign policy decision-making matrix (p. 4)—management—is overlooked. Crucial decision-making distortions in foreign policy—such as groupthink, disjointed incrementalism, and satisficing—similarly receive little treatment, and the crisis decision-making literature is barely mentioned.

As with many edited collections, the chapters are of uneven quality. Several provide innovative and well-grounded insights on narrow slices of foreign policy decision making. For example, Richard Herrmann presents a convincing case as to why there should be more attention paid to perceived opportunity (rather than perceived threat) as a foreign policy motivation. Margaret Hermann takes an intriguing look at how leaders' sensitivity to their environment and interest in foreign affairs affect their states' foreign-policy behavior. Philip Tetlock compares American senators and British parliamentarians to show that extreme ideologues possess lower belief complexity than do moderates. And, by exploring the relationship between public beliefs and foreign policy behavior during the Carter era, Jerel Rosati finds that the correlation is highest when consensus prevails among the top foreign policy makers. But Susan Kisiel's chapter on voice stress analysis is too preliminary and descriptive, and John Lovell's chapter on bureaucratic learning contains poorly formulated hypotheses. Furthermore, the entire section on artificial intelligence is weak. Two of its chapters, by James Bennett and Akihiro Tanaka, seem to discuss complex computer models rather than artificial intelligence, which unlike simulation requires that the computer itself (rather than human programmers) generate and "learn" rules of behavior through trial and error.

The isolated analytical strands within this collection cry out for integration and synthesis both with each other and with others in the foreign policy-making literature. Implied controversies run rampant throughout this volume. Can foreign policy wisdom be learned? Do leaders' beliefs have a greater impact on foreign policy behavior than this behavior has on these beliefs? Although any collection on foreign policy decision making might raise more questions than it answers, these controversies need more open-minded identification and examination. Despite its flaws, this book has accomplished perhaps its greatest service by opening up more cans of worms.

ROBERT MANDEL

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Required by Act of October 23, 1962:
Section 4369, Title 39, United States Code

1. **Date of Filing:** October 30, 1985.
2. **Title of Publication:** The American Political Science Review.
3. **Frequency of Issue:** Quarterly in March, June, September, December.
4. **Location of Known Office of Publication:**
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
5. **Location of the Headquarters or General Business Office of the Publishers:**
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
6. **Names and Addresses of Publisher and Managing Editor:**
   Publisher: American Political Science Association,
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
   Managing Editor: Samuel C. Patterson,
   Department of Political Science, University of Iowa, Iowa City, IA 52242.
7. **Owner:**
   American Political Science Association, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
8. **Known Bondholders, Mortgagees, etc.:** None.
9. The purpose, function and nonprofit status of this organization and the exempt status for federal income tax purposes have not changed during the preceding 12 months.
10. **Extent and Nature of Circulation:**

	Average No. Copies Each Issue During Preceding 12 Months	**Actual No. Copies Single Issue Nearest to Filing Date**
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2. Mail Subscriptions	12,058	11,964
C. Total Paid Circulation	12,058	11,964
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E. Total Distribution (C + D)	12,125	12,031
F. Office Use, Left-Over, Unaccounted, Spoiled after Printing	1,811	1,929
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Ira H. Carmen

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Ira H. Carmen, a member of the Political Science department of the University of Illinois, is author of two books on constitutional subjects — *Movies, Censorship, and the Law* (University of Michigan Press, 1966) and *Power and Balance* (Harcourt Brace Jovanovich, 1978) — and numerous articles in law reviews and other professional journals.

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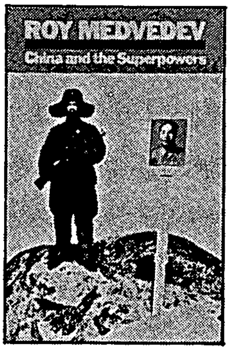

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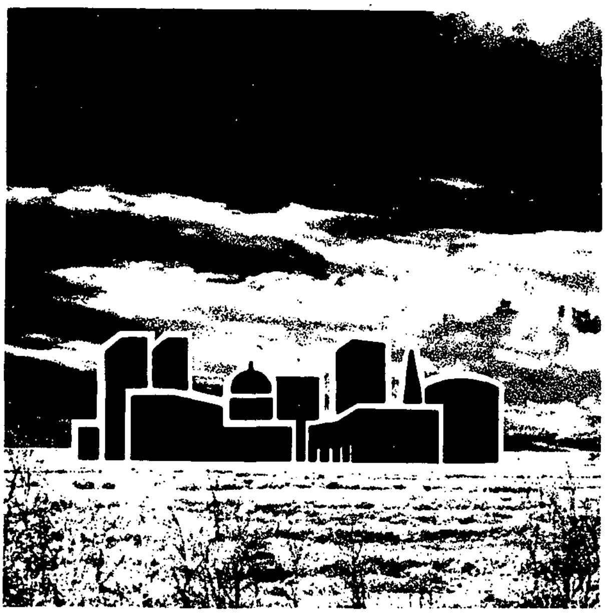

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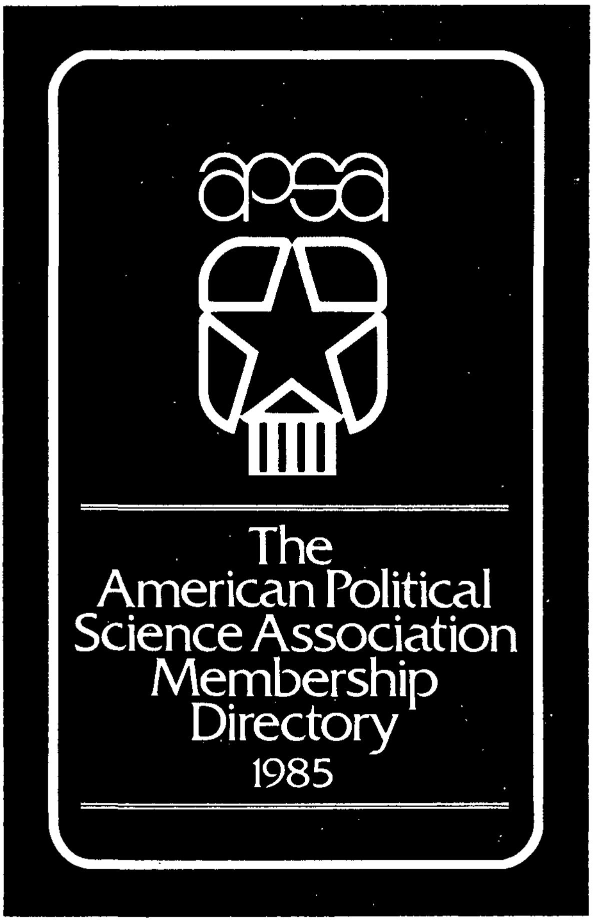

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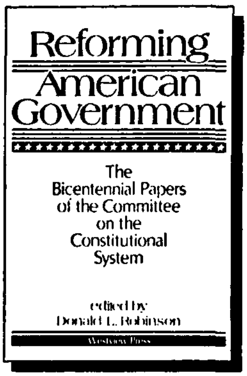

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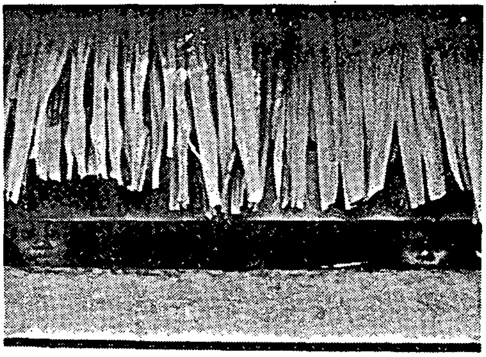

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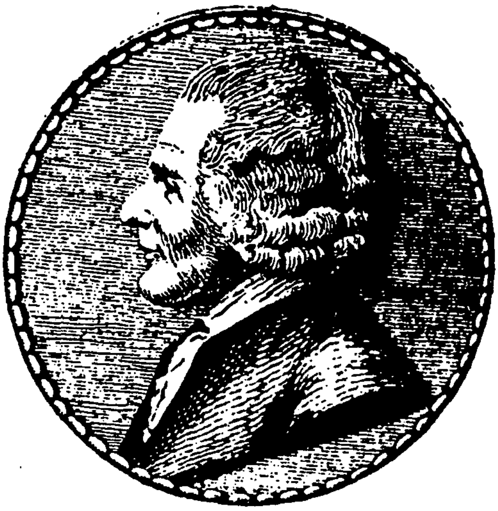

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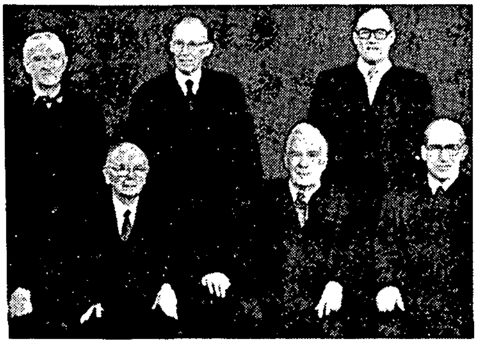

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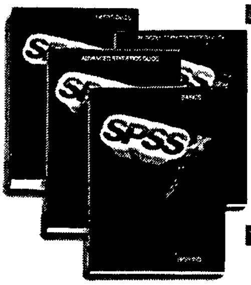

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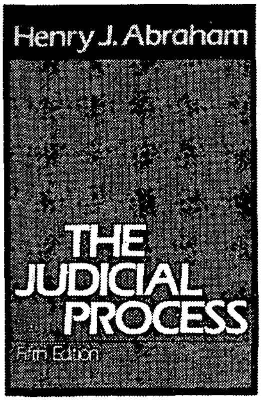

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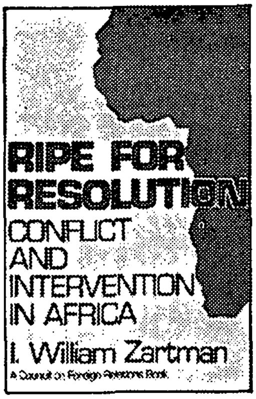
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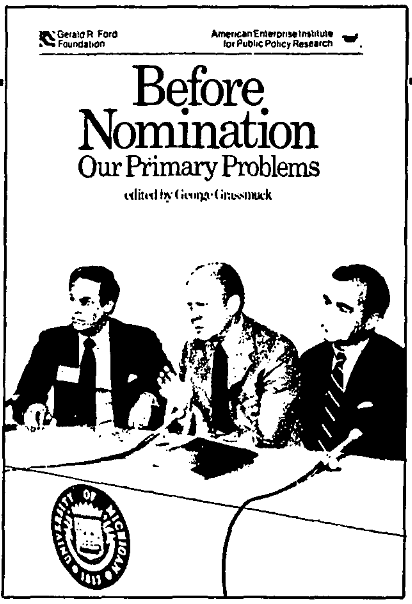

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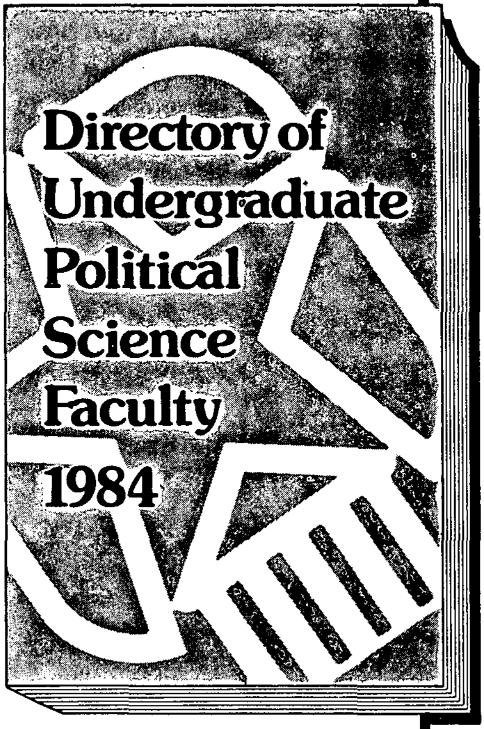

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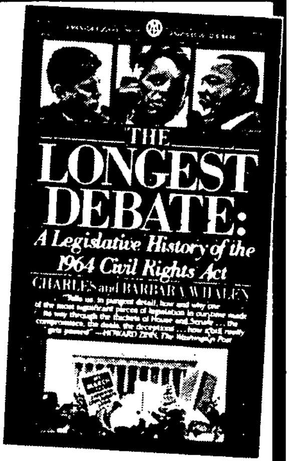
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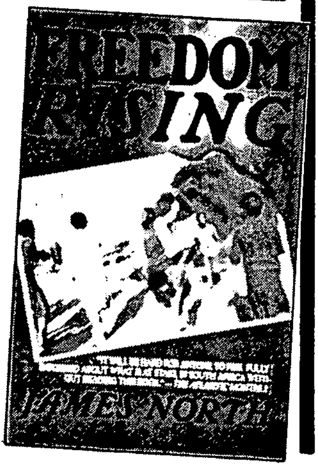

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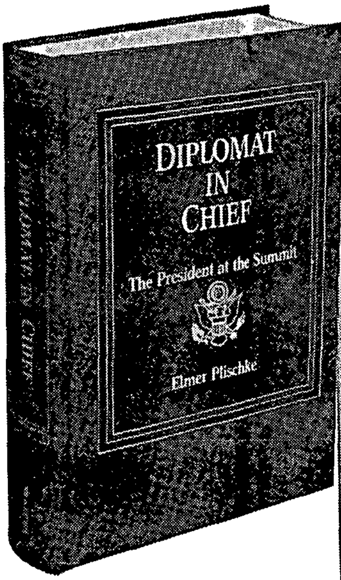
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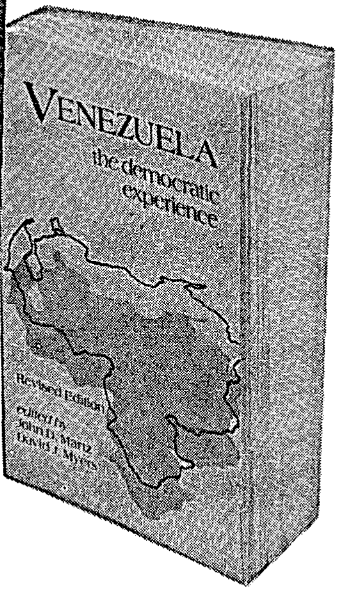
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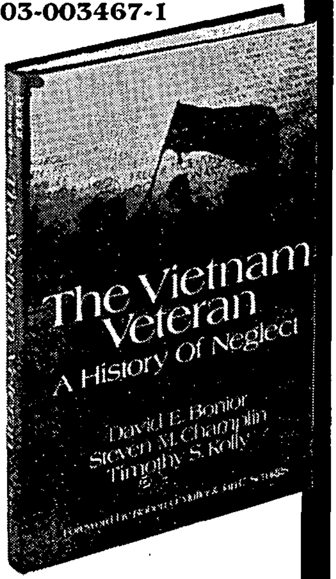
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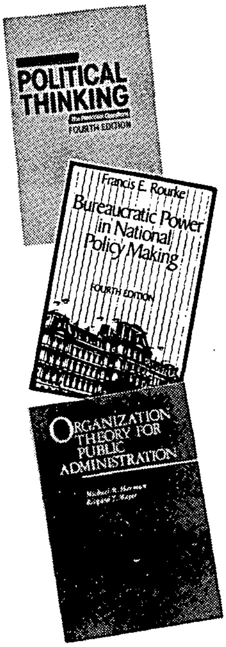
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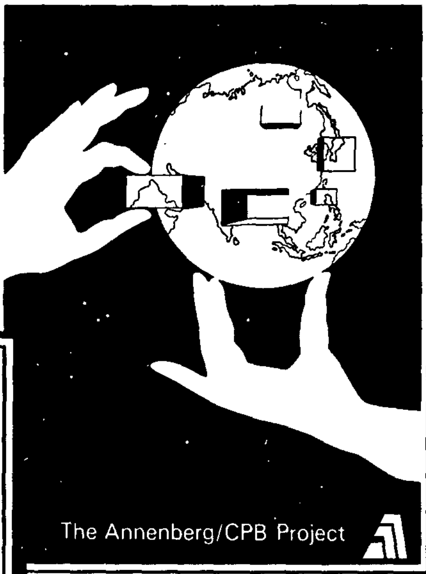
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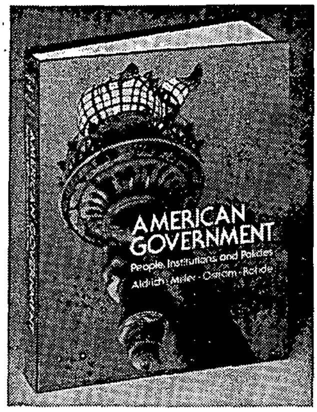
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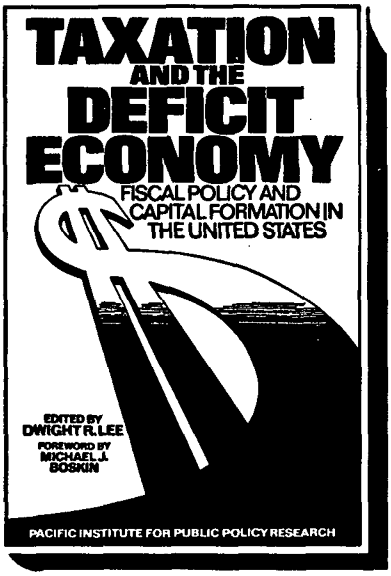

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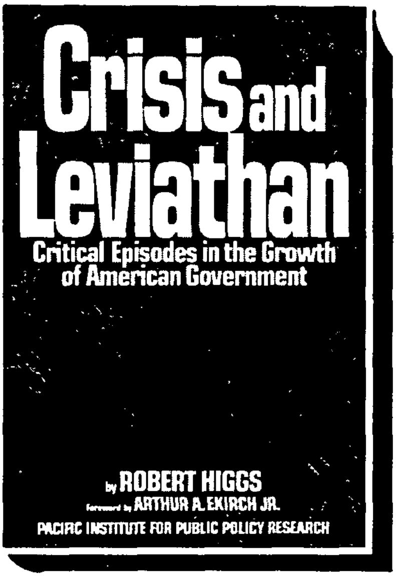

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